ART. IV.—1. *Fundamenta Astronomiæ pro anno* MDCCLV, *deducta ex observationibus viri incomparabilis James Bradley in Specula Astronomica Grenovicensi per annos* 1750—1762 *institutis.* Auctore FRIDERICO WILHELMO BESSEL, Acad. Berol. Atque Petrop. Sodali, Instituti Gallici Corresp. Regiomonti, 1818. T. 1. pp. 328.

2. *Tables Astronomiques publiées par le Bureau des Longitudes de France, viz.*

Tables de La Lune. Par M. BURCKHARDT, Membre de l' Institut, etc. Paris. 1812.

Nouvelles Tables de Jupiter et de Saturne, calculées d'apres la théorie de M. Laplace, et suivant la division décimale de l'angle droit. Par M. BOUVARD. Paris. 1808.

Tables écliptiques des Satellites de Jupiter, d'apres la théorie de M. le Marquis de Laplace, et la totalité des Observations faites depuis 1662 *jusqu' à l'an* 1802. Par M. DELAMBRE. Paris. 1817.

3. *Tables.* By B. DE LINDENEAU, viz.

Tabulæ Veneris novæ et correctæ, etc. Gothæ. 1810.

Tabulæ Martis novæ et correctæ, etc. Eisenberg. 1811.

Investigatio nova orbitæ a Mercurio circa solem descriptæ accedunt Tabulæ Planetæ, etc. Gothæ. 1813.

4. *Mémoire sur la figure de la Terre.* Par M. DE LAPLACE, Mem. Acad. Sciences. Paris. 1817, 1818.

THE science of Astronomy offers to our contemplation some of the most powerful efforts of the human mind. Copernicus, by the discovery of the motion of the planets about the sun; Kepler, by his elliptical theory, and the laws regulating the motions and distances of the planets, with the times of their periodical revolution; finally, Newton, by the discovery of the theory of gravity, opened the way for all the improvements, which have lately been made in this science. In the *Principia*, published in 1687, Newton pointed out the origin of the inequalities of the motions of the heavenly bodies, which had then been discovered by observation, and deduced others from the theory of gravity. No material alteration was made in his methods for more than half a century. Then began a new epoch in Astronomy, and the history of that science, for the last hundred years, will be forever memo-

rable for the unexampled activity and great discoveries, which have been made. So important have been the labors of the practical astronomers, that a complete system of the planetary motions might be deduced from the observations made during this time; and if all the previous observations, even to the most remote antiquity, were lost, the effect on the tables of the sun, planets, and satellites, would hardly be perceived, since the great accuracy of modern observations more than compensates for the shortness of the interval. It is proposed in this article to give a short account of some of the most noted discoveries during this period, to take a slight view of the latest and most correct tables of the motions of the planets and satellites, and to make such remarks on the labors of astronomers and mathematicians, as may be necessary in the notices of the works proposed to be reviewed.

The career of modern improvement was begun by Dr Bradley, one of the most indefatigable astronomers of the last century. He was remarkable for his skill and accuracy, in tracing those minute changes in the places of the heavenly bodies, which had so much perplexed the astronomers who preceded him, and his labors were crowned with the most brilliant success, by the discovery of the Aberration of light and the Nutation of the earth's axis. His observations were so numerous, accurate, and important, that he may justly be placed in the same rank with Hipparchus and Tycho, the greatest and most accurate observers of ancient and modern times. He published an account of the aberration and nutation. His observations of the moon were also made public, and used by himself and others, in comparing and improving the lunar tables. A table of the places of 389 fixed stars was likewise deduced from his observations, and published by Dr Hornsby, but the great body of his observations, made at Greenwich while he was astronomer royal, were taken from the observatory by his executors, under the pretence that they were his private property, with the expectation of being paid for them by the government. A suit having been commenced for their recovery, the executors, in order to avoid it, presented them to Lord North, (so well known in the history of the American Revolution,) who gave them, in the year 1776, to the University of Oxford, of which he was Chancellor, upon the express condition, that they should immediately be

printed and published. But to the great disgrace of the University, and of Professor Hornsby, who had charge of the papers, they were withheld many years, notwithstanding the repeated solicitations and remonstrances of the Board of Longitude, who, in 1796, published several spirited resolutions, under the form of an appeal to the public, upon this very improper conduct. These observations were made between the years 1750 and 1762, but it was not till the year 1798 that the first volume was published, and the whole was not completed till the year 1805, almost half a century after the observations had been made; and during the whole of this time, while unexampled progress was making in all branches of astronomy, these invaluable observations, which would have facilitated very much the calculations of astronomers, were lying almost useless.

But it may well be questioned whether this delay will, on the whole, be any disadvantage to the future progress of astronomy. For if these observations of the stars had been published soon after Bradley's death, they could not then have been reduced so accurately, as at the present moment, because the precise values of the small reductions to be made to the observations for precession, nutation, aberration, and refraction, were not so well known, and it was not then usual to take such pains in computing and combining together many observations. Moreover, if the great labor of reducing the observations had been once gone through, even in a somewhat imperfect manner, it is probable that no one would have undertaken a new revision, as is the case with Flamsteed's observations. But, at the time of the publication of the observations, a considerable degree of interest had been excited, from he difficulties attending them, and this, with the well known accuracy of Bradley, was sufficient to procure an early and careful examination. Fortunately, at that time Bessel, the present astronomer royal at the observatory of Konigsberg, had just relinquished his mercantile pursuits, and with great success had devoted himself to astronomy. Having been furnished with a copy of Bradley's observations by Dr Olbers, he voluntarily undertook the task of reducing them and no one was better qualified to do it, since he possessed, what is rarely united in the same individual, mathematical talents of the very first order, with great accuracy in

observations. The result of his labors is the important volume mentioned at the head of this article.

This work is divided into thirteen sections, in which Bessel successively treats of the various subjects connected with Bradley's observations, namely, the Instruments he used, and the corrections to be made to them. The Right Ascensions of his fundamental stars compared with the sun near the equinoxes. The Latitude of Greenwich. The Refraction of the heavenly bodies, deduced solely from Bradley's observations, combining them together by an excellent theory, and with tables for the calculation, being more accurate than any tables of refraction, that had before been used by astronomers. The Obliquity of the Ecliptic from the observations of the solstices from 1753 to 1760. The Aberration of the fixed stars, with tables peculiarly adapted to the reduction of Bradley's observations, and an investigation of the quantity of the aberration, deduced from a great number of those observations, by which it would seem that the value of the aberration as found by Delambre, from the Eclipses of Jupiter's satellites, ought to be increased about a fortieth part. The Precession of the Equinoxes and Nutation. The Parallax of the fixed stars, which, by comparing a great number of Bradley's observations of the right ascensions of two stars on opposite meridians, (by which the effect is nearly doubled,) seems to be insensible.

But the most important part of the work is his excellent catalogue of 3222 fixed stars, in which the situation of each star is most commonly ascertained by several observations. In this catalogue he has given Flamsteed's numbers, their characters and magnitudes, also their right ascensions and declinations for the year 1755, with the annual precession for 1755 and 1800. The differences between the places of the stars and those in Piazzi's catalogue are likewise noted, with various references to other authors, who have observed the same stars. To this table is subjoined a smaller one of *forty eight* stars, observed by Bradley, which cannot now be found in the places where he had marked them. Several were, without doubt, inserted by mistake, like that of writing down a wrong hour or minute of the time of observation, as is evident from the remarks on this table by Bessel, Burg, and Burckhardt, in Zach's *Monatliche Correspondenz.* One

of these missing stars was, however, the planet Uranus, which was observed by Bradley, Dec. 3, 1753, and marked as a fixed star, without the least suspicion of its being a planet; he being less fortunate in this respect than Herschel, who, about thirty years afterwards, by repeating his observations on successive nights, detected its planetary nature by the change of place. Finally, Bessel devotes one of the sections of his work to the consideration of the proper motions of the *fixed* stars, and pursuing the observation of Herschel, directs his attention, particularly, to the double stars, some of which indicate a *mutual attraction between each other, and a revolution about their common centre of gravity.* This is particularly the case with the star 61 Cygni, which is estimated by Bessel to perform its revolution in 350 years. The double star ξ Ursæ Majoris, in 60 years, the double star p 70, Serpentarii, in about 50 years, and many others, noticed by Herschel and since by Struve, who has lately made many observations on such stars at the observatory of Dorpat.* J. W. Herschel has also given a valuable paper on these stars.

Bradley's chief excellence consisted in noting, with unexampled accuracy, the times of the transits of bodies over the meridian and their zenith distances, and he was not remarkable for noticing celestial phenomena of a different nature, neither were his mathematical talents of the first order.

To form some idea of the accuracy of Dr Bradley's observations, and to shew at the same time what is now required of a first rate observer, it is only necessary to compare the results of the transits of fixed stars of the first and second magnitude, observed during one night, for the purpose of fixing the rate of the clock. From the mean of *twelve* observations of this kind, Bessel found the error of the clock to be about seventeen seconds and one *fifth* part of a second; *nine* out of *twelve* observations did not differ *one tenth* of a second from the mean result, and the greatest difference did not exceed *one third* part of a second. The same degree of accuracy exists also in his right ascensions of the stars, since

* It is rather strange that one of the best Observatories in Europe, as that at Dorpat undoubtedly is, should be situated in so high a latitude, being on the same parallel with the cold regions of Siberia. Notwithstanding this, the indefatigable Struve, overcoming the difficulties of the climate, has, in the course of a few years, published several volumes of excellent observations, which he has made at that place.

the results of successive years, when reduced to the same epoch, differ from each other but a small fraction of a second. His measures of zenith distances of the heavenly bodies were equally correct. An instance of which may be mentioned in the obliquity of the ecliptic for January 1, 1755, determined by the observations of fifteen solstices, from 1753 to 1760, to be 23° 28 15.″44, and *eleven* out of the *fifteen* observations did not differ a single second, and the extreme difference was less than 3 seconds. Moreover, he found the observations of the summer solstices gave the same result as those of the winter, and, in this respect, his observations were free from the noted error, which existed for many years in those of his successor, Dr Maskelyne, who found, about the year 1795, the summer solstice gave for the obliquity 4 or 5 seconds more than the winter solstice, and a similar difference having been observed about that time by Piazzi, the question was started and much discussed, to account for this difference, and various hypotheses were proposed for that purpose. Among them the one that seemed most plausible was, that the refraction of the *sun's* rays was different from that of the *fixed stars*, and, as the tables of refraction were founded on observations of the stars, a modification was proposed for solar observations. This discussion continued several years, and the true cause was not discovered, till Bradley's observations were published. It was then found by Bessel, that no such difference existed in the observations made by Bradley, when the instrument was new; that the error was not perceptible till the instrument had been used many years by Dr Maskelyne, and had become defective by constant use, so that at length there was an error of nearly 3″ in the measure of these angles. Upon procuring a new circular instrument, this difference in the observations of the solstices ceased, and astronomers were enabled to determine the obliquity to a great degree of accuracy, which is a very important point, since this element enters in some way or other into almost every calculation of astronomy, and a change of a few seconds would, in some cases, affect the calculations considerably.

While Bradley was making his observations in Greenwich, his cotemporary, Tobias Mayer, was devoting his short, but extremely laborious and useful life, to the same pursuits in Got-

tingen, and with great success. Mayer's lunar tables, for which his widow received a large reward from the Commissioners of Longitude of Great Britain, first gave the moon's place in the heavens with sufficient accuracy to determine the longitude at sea, which has conduced so much to the safety and rapidity of modern navigation, and to the immense improvements, which have been made in geography within the last fifty years. Mayer possessed much more mathematical knowledge than Bradley, but had not his fine *tact* in observing, neither were the instruments he used so perfect.

About the same time that Mayer and Bradley were observing in the northern hemisphere, La Caille, at the Cape of Good Hope, was forming his catalogue of ten thousand stars of the southern hemisphere. His labors were immense, and it has been asserted, that he made more observations and calculations, than all the astronomers of his time taken together.

The taste for making improvements in the instruments and in the methods of observing, which began with Dr Bradley, has continued to the present time, and a regular series of observations has been obtained, from which new and complete tables of the motions of the planets and satellites have been formed, exceeding all expectation; so that an astronomer could now predict, for a thousand years to come, the precise moment of the passage of any one of those bodies over the meridian wire of the telescope of his transit instrument, with such a degree of accuracy, that the error would not be so great as to remove the object through an angular space corresponding to the semidiameter of the finest wire that could be made; and a body, which by the tables ought to appear in the transit instrument in the middle of that wire, would in no case be removed to its outer edge. In this work of improvement there were many cooperators, and the artist, by the perfection of his instruments, the astronomer by his observations, and the mathematician by his analysis, have mutually assisted each other. With the excellent instruments, made by Graham, Dr Bradley discovered that apparent motion of the fixed stars, which depends on the nutation of the earth's axis, and soon afterwards D'Alembert explained, upon correct principles, the physical causes of that motion, and gave formulas for computing it, shewing at the same time,

that the apparent motion of the pole of the earth was not in a circle, as Bradley and Machin had supposed, but in an ellipsis of considerable eccentricity. Many instances of a similar nature have occurred during the last century, and it has almost always happened that the English have furnished the best artists, the best instruments, the best practical astronomers, and the best observations, except in the case of the four small planets, lately discovered, while their continental neighbors, particularly the French and Germans, have made the improvements in analysis, and the deductions from the English observations, which were necessary for the computation of the present accurate tables of the motions of the heavenly bodies.

The decided superiority of the English artists in the construction of astronomical instruments, for the measure of angles, has been generally acknowledged by all the astronomers of Europe. So long ago as the year 1736, when the French Academicians were sent to the north to make observations for ascertaining the figure of the earth, the famous English artist, Mr Graham, was thought the fittest person in Europe to supply them with instruments. No greater proof of his superiority could have been given, than his being thus employed by the ministers of a rival nation in a work of such celebrity. Graham and Bird furnished the instruments for the observatory of Greenwich, when Bradley was appointed astronomer royal, and the same instruments were afterwards used by Dr Maskelyne in making his important observations. Bird's mural quadrants were famed over all Europe for their accuracy. He made them for the observatories of Greenwich, Paris, Petersburgh, Oxford, Manheim, Gottingen, Cadiz, &c. To him succeeded Ramsden, whose skill as an artist far surpassed that of any other man of his time. The mural quadrant he made for the observatory at Blenheim, was considered a most excellent instrument, and his meridian circles were still more complete. He made one of these circles for Piazzi at Palermo, with which that celebrated astronomer made the observations for his great catalogue of the fixed stars. He also made that, which Dr Brinkley is now so successfully using at the observatory of Dublin. His great theodolite, used by General Roy in the survey of the English coast, is famed for its accuracy and completeness.

Ramsden invented the dividing machine to graduate the arcs of sextants, and made equatorial instruments upon a large and much improved plan. Having married a daughter of John Dollond, he became possessed of a part of his patent right for the manufacture of achromatic telescopes, first brought into use by Dollond, by a method of construction to which he had been led by the suggestion of Euler, for correcting the colored images in a somewhat similar manner. The application of these telescopes to transit instruments was an important improvement. Ramsden furnished the observatories of Blenheim, Manheim, Dublin, Paris, Gotha, &c. with some of these meridian telescopes, remarkable for the excellence of their object glasses. With that in the Dublin observatory stars of the fourth magnitude may be seen, when passing the meridian, in the open day, and those of the third magnitude even when very near their conjunction with the sun. He carried the principle of division of labor to a great extent, and while employing above sixty persons, he always confined the same workmen to the same branch, and by that means attained the greatest correctness and nicety in the execution. His instruments were in such demand, in every part of the world, that he was unable to execute all the orders he received, and it was not uncommon to be obliged to wait for them several years. The flights of his genius were as uncertain as those of a poet, and not to be regulated by times and seasons. He usually made a model of any important instrument, in which a new principle of construction was introduced, and would take apart his favorite foreman to set down leisurely 'and find fault with it,' and if any defects were discovered, some new method was adopted.

After the decease of Mr Ramsden, which happened in 1800, Mr Troughton was considered the most skilful artist of the kind in England. He made many excellent instruments. His *chef-d'œuvre* is the complete circle, made for the observatory at Greenwich, and fixed up since Mr Pond was appointed astronomer royal. It is said that nothing can exceed the accuracy of this instrument. Advanced age makes him unwilling to undertake to construct others of the same kind. For, upon being applied to by Harvard University, to make a circle and sextant exactly similar to those

just erected at Greenwich, he declined, observing jocosely to some one, that it was impossible, unless he could obtain a *new lease* of his life. He made several of the beautiful and accurate instruments for the government of the United States, under the direction of Mr Hasler. It is to be regretted, that no better use is made of them than to lock them up, after some have been spoiled, like articles of curiosity in a museum. The person who seems destined to take the place of Mr Troughton is Mr Thomas Jones, who has already made several valuable instruments, particularly those for the new observatory, erected by the English government, at Parramatta in New South Wales.

About the commencement of the present century, Reichenbach, in Germany, began to make excellent astronomical instruments of various kinds, which he sold at a moderate rate, and supplied several observatories on the continent of Europe. The graduation of his instruments was made with the utmost accuracy, and would compare in every respect with the works of the best English artists. His repeating astronomical circles have been highly spoken of. This instrument was invented by Borda, and much used by the French astronomers, particularly in their late measurement of the earth, between Dunkirk and Barcelona. Reichenbach has lately retired from business, and established himself at Vienna with a different occupation.

The optical instruments of Fraunhofer in Germany are highly recommended. The achromatic telescope, which he has lately constructed for the observatory of Dorpat, of above 14 feet focal length and 9 inches aperture, is spoken of as one of the most complete instruments of the kind ever made.

The English have not less excelled in the construction of Instruments of Reflection, so necessary for finding the latitude and longitude at sea. They were first brought into use by Mr John Hadley. The invention has also been claimed for our countryman, Thomas Godfrey. The truth, however, is, that neither of them was the *first* inventor, for Newton, many years before either of them, had explained the principles of this instrument, in a paper in his own handwriting, communicated to Dr Halley, which was found after his death, and laid before the Royal Society of London in 1742. The instruments were first made in the form of an octant, after-

wards, for the purpose of lunar observations, the arch was extended to a sextant. Finally, to obtain a multiple angle, Mayer proposed to make them of a circular form, and this construction was afterwards improved by Borda and others. In general, the circular instruments of Borda's form *have not been well made in England*, it has not been a favorite with them. The construction of Troughton, in which the principle of obtaining a multiple angle is lost, has been preferred, and English circles of this form have generally been found to be excellent.

The English have likewise excelled very much in the construction of chronometers and clocks. The rewards granted by the British government, for the improvements in the construction of chronometers, have been very splendid. Harrison received twenty thousand pounds sterling, Arnold, Earnshaw, and Mudge, each three thousand pounds. Several who had made improvements did not apply for the reward promised by the Act of Parliament, as Brockbank, Hardy, Emery, Grimalde, &c. In this department the French have likewise produced eminent men, as Le Roy, Berthoud, &c.

In the construction of reflecting telescopes, Herschel, a German by birth, but an Englishman by adoption, excelled all others in the great magnifying powers of his instruments, with which he made his important discoveries. No telescopes so powerful as his were made in any part of the continent of Europe, and it is only with such instruments that some of his observations can be repeated. For instance, the satellites of Uranus cannot be seen, except by one of his most powerful reflectors, so that but very few astronomers have an opportunity of viewing them. Previous to Herschel's time, Short had made many valuable telescopes, particularly one for the King of Spain in 1752, for which he received twelve hundred pounds sterling; but not one of these instruments would compare with the large ones made by Herschel.

With the assistance of such artists, it was to be expected that the English astronomers would excel all others in their observations of the heavenly bodies, and such has been the fact, except in the observations of comets and the new planets. Indeed, the single observatory of Greenwich has alone done more for the improvement of astronomy, since the time of its erection in 1675, than all the other observatories in

Europe taken together, and what Baron de Zach observes upon this subject is not much exaggerated, when he says, 'that if any one should assert that our astronomical tables would be equally perfect, if the other *hundred and thirty* European observatories had never existed, he would be very well able to support his assertion, although at first view it might appear quite extravagant.' The instructions given to the first astronomer royal, and to his successors were, 'That they should apply themselves with the utmost care and diligence to verify the tables of the motions of the heavens, and the places of the fixed stars, in order to find the so much desired longitude at sea for the perfecting of the art of navigation.' These instructions have been most faithfully obeyed, by furnishing an almost uninterrupted series of the best observations for one hundred and fifty years.

Flamsteed received the first appointment of astronomer royal at Greenwich in 1675, and continued till his death in 1719. The result of his labors was published in his *Historia Cœlestis Britannica* in 1725, in three large folio volumes, containing, besides other matter, his observations on all the heavenly bodies, and a catalogue of the right ascensions, polar distances, magnitudes, &c. of about 3000 fixed stars, with a preface giving an account of the observations made before his time; a new Latin version of Ptolemy's catalogue of 1026 stars; and Ulegh-Beig's places annexed; a small catalogue of the Arabs; Tycho Brahe's of about 780 fixed stars; the Landgrave of Hesse's of about 386; Hevelius's of 1534; and a catalogue of some of the southern fixed stars not visible in our hemisphere, calculated from the observations made by Dr Halley at St Helena, adapted to the year 1726. Flamsteed's observations were made with the best instruments then in use, particularly a large mural arch of seven feet radius, but none of them would compare in accuracy with those afterwards introduced by his successors, and on this account his labors are much less important than they otherwise would have been. Moreover, the aberration and nutation not having been discovered and allowed for, when his observations were reduced, they are liable to the errors arising from these causes.

Upon the death of Flamsteed, in 1719, Dr Halley was appointed to succeed him, being then about 63 or 64 years of age.

For the space of eighteen years he watched the heavens with the closest attention, devoting himself particularly to make observations of the moon during the period of a revolution of the nodes, which he completed notwithstanding his great age. His observations were not published, but from them and Flamsteed's he formed tables of the motions of the heavenly bodies, which, for nearly fifty years, were the best extant. It was he who first proposed to determine the sun's parallax by the transit of Venus in 1761, and he was the first who predicted the return of the comet, which bears his name. In the year 1676, when only twenty years of age, he went to St Helena to make a catalogue of the southern stars, which he completed in about two years. In the year 1699, he made another voyage, in a public armed vessel, under his command, (having been appointed without any previous service, contrary to the established *etiquette* in the navy,) in order that he might go to the Southern Ocean to observe the variation of the magnetical compass, upon which he had before given a theory, and, in 1701, he published a general chart, showing the variation of the compass in most parts of the world. To Halley, the world is, in some measure, indebted for the publication of Newton's Principia; it was done at his intercession; he had the whole care of the first impression, and prefixed to it a discourse of his own, giving a general account of the astronomical part of the work.* He published many valuable papers on almost every subject of science; as an engineer he was extremely skilful, and was employed by the emperor of Germany and other sovereigns of Europe. His salary, as astronomer royal, was merely one hundred pounds sterling per annum, being the same that Flamsteed had received. Upon a visit, that Queen Caroline made to the observatory, it was proposed to have this sum increased, but his answer was, 'I pray your Majesty do no such thing;

* It is a curious fact, that when the Jesuits, Le Seur and Jacquier, published their edition of Newton's Principia with notes, they had virtually to disavow the belief in the earth's motion about the sun, and, like Galileo, were almost compelled to consider it as a damnable heresy, according to the previous decisions of the sovereign Pontiff. The following declaration is in the third volume of their edition, printed at Geneva in 1742. 'Newtonus in hoc tertio libro telluris motæ hypothesim assumit. Autoris propositiones aliter explicari non poterant, nisi eâdem quoquè factâ hypothesi. Hinc alienam coacti sumus gerere personam. Cæterum latis à summis Pontificibus contrà telluris motum Decretis nos obsequi profitemur.'

for should the salary be increased, it might become an object of emolument to place there some unqualified, needy dependant, to the ruin of the institution.' This advice was the more disinterested, as Halley was not rich. He died in the year 1742, at the age of 86 years.

Dr Bradley succeeded Dr Halley, as astronomer royal, February 3, 1742, but the imperfections of the instruments prevented him from doing much till the year 1750, when the new instruments made by Graham and Bird were fixed up. Those made by Bird were a transit instrument of 8 feet radius, a mural quadrant of 8 feet, and a portable quadrant of 40 inches; those by Graham were the 12 feet zenith sector, with which Dr Bradley had discovered the aberration and nutation at Wanstead, an astronomical clock, and an equatorial sector. There was also a six feet Newtonian telescope by Short. With these excellent instruments Bradley observed the heavens till the time of his death, which happened twelve years afterwards, and with the help of his assistants Charles Mason, and his nephew John Bradley, observed 35,000 transits of the stars and planets over the meridian, 24,000 zenith distances with the quadrants, and 1500 zenith distances with the sector. These are the observations which Bessel reduced, as has been already mentioned, and none comparable to them in accuracy had ever before been made. Dr Bradley may be considered as a perfect model of an observing astronomer. He had a robust constitution and was extremely active. He was mild and gentle in his manners, silent, retiring, and industrious; he had taken orders in the church, and during his residence at the observatory the living of the church at Greenwich became vacant and was offered to him, but was refused from a conscientious scruple, 'that the duty of a pastor was incompatible with his other studies and necessary engagements,' upon which occasion the king granted him a pension of two hundred and fifty pounds sterling in addition to his original salary, which has been since regularly continued to the astronomer royal.

His successor, Dr Bliss, was wholly unworthy of the office of astronomer royal. The account of his life by La Lande is comprised in less than a dozen words, 'Bliss était astronome royal; il mourut en 1765.' It is almost inconceivable how any one, following so immediately after Bradley, could

have caught so little of his spirit. During the thirty months in which he held that office, he made no observation with the zenith sector, and only 41 zenith distances of the stars with the quadrants. None of these observations could be called good. The instruments seemed almost to have changed their nature under his direction, and the clock, which, with Bradley's careful management, went better than ever had been known in any observatory, became, by his carelessness, an indifferent measurer of time.

Dr Maskelyne was appointed astronomer royal in 1765, and continued in the office till his death in 1811, a period of 46 years, with the highest credit to himself and honor to his country. Like Flamsteed and Bradley he had taken orders in the church, and, early in life, officiated in the curacy of Burnet. In his manners he was modest, simple, and unaffected, and exemplary in the discharge of every duty.

In 1761 he went to St Helena to observe the transit of Venus, and in the course of that voyage, and of one he subsequently undertook to Barbadoes to determine the rate of going of Harrison's time keepers, he exercised himself and the officers in taking 'lunar observations,' and was the first who brought that method into common use, by procuring the publication of the Nautical Almanac, the first number of which was published in 1767, and he lived to complete the fiftieth number, a lasting monument of his labor and usefulness. He likewise published the 'British Mariner's Guide,' and the 'Requisite Tables,' which contain the principles of the improved methods of finding the longitude at sea. He will, however, be most known to posterity by his observations on the heavenly bodies, the first volume of which was published in 1765, and they have been continued annually since. This important collection of about 90,000 transits over the meridian, and 26,000 zenith distances, has been justly called by La Lande, 'le recueil le plus précieux que nous ayons.' It is sufficient to say that the observations are equal to Bradley's. The solar tables of Delambre, and the lunar tables by Burg and Burckhardt, are grounded chiefly upon them.

Upon the publication of those tables, in 1806, the Board of Longitude of France ordered a number of copies to be presented to Dr Maskelyne, as a grateful acknowledgment for the important assistance derived from the Greenwich ob-

servations. The letter accompanying this present is highly honorable to that Board, as well as to Dr Maskelyne, taking into consideration that the two rival nations were then carrying on the war with a bitterness, which allowed but very little intercourse with each other.*

Dr Maskelyne devoted much time to the improvement of his catalogue of the right ascensions and declinations of 36 principal fixed stars, called 'fundamental stars,' or '*Dr Maskelyne's 36 stars*,' which were used by all the astronomers of Europe in forming their catalogues, or in ascertaining the right ascensions and declinations of other fixed stars, by observing the differences in right ascensions and declinations. This catalogue was the basis of all the calculations of astronomers. The place of every fixed star in the heavens, and all the tables of the planets and satellites depended upon it. It was believed to be free from any sensible error, and astronomers were much surprised in the year 1802, when Dr Maskelyne proposed to increase all the right ascensions by *one quarter of a second* of time, or about four seconds of space. Delambre speaking of this subject says, 'Ce changement, qui tient à une si petite fraction, causa cependant *une espèce de fermentation dans les esprits* des astronomes.' Nothing could place in a stronger point of view the general confidence in the accuracy of Dr Maskelyne's table, than the excitement caused by a change of this kind, which was in fact so small as to be almost imperceptible to the senses. For though in observations the time is usually marked to tenths of a second, for the

* The following is a copy of this letter;

'Institut National, Classe des Sciences Physiques et Mathématiques, Paris, le 20 Fevrier, 1806. Le Secrétaire perpétuel pour les Sciences Mathématiques à Monsieur Maskelyne, Astronome Royal et Membre de la Société Royale de Londres. Monsieur, et respectable Confrère.

Le Bureau des Longitudes me charge de vous offrir sept exemplaires des tables qu'il vient de publier. Cet hommage de sa haute estime et de sa reconnoisance étoit bien dû à l'auteur *du plus grand et du plus précieux recueil d'observations qui existe.* C'est à cette source que nous avons puisé Monsieur Burg et moi pour la plus exacte determination des coefficiens des equations lunaires et solaires, c'est là que nous avons trouvé la confirmation des inégalités que la théorie peut bien indiquer, mais dont la valeur ne pourrait etre fixée que par des calculs qui sont encore au dessus des forces de l'analyse; enfin *c'est à vous que nous devons la connoisance des mouvemens moyens* et de toutes les constantes que l'observation seule peut donner. Recevez donc avec bienveillance, un ouvrage auquel vous avez si puissamment contribué. Nous serons très flattés si vous jugez nos tables dignes d'être employées aux calculs du Nautical Almanac, suivant l'apparence que nous en donne votre dernier préface.' DELAMBRE.

convenience of the decimal notation, yet it is not pretended to estimate it to a greater degree of accuracy than two tenths or one fifth of a second, which can easily be done, since we can very distinctly count *five* in one second of time. A similar discussion is now carried on between Pond, Bessel and Brinkley, relative to the declinations given in this catalogue. All three of these astronomers have observed the places of these stars, with the best instruments that can be made, and yet the declinations of Pond and Bessel differ nearly four seconds throughout the whole catalogue. No satisfactory explanation has yet been given of this remarkable fact. It is some consolation, however, to find the error reduced within the narrow limits of *four* seconds. Among the ancients, even when astronomy had made considerable progress, as for example, in the time of Hipparchus, an error of *four thousand* seconds was not uncommon in some of their observations.

Upon the death of Dr Maskelyne, in 1811, Mr Pond was appointed astronomer royal. Since his accession three new instruments made by Troughton, with all the latest improvements in the construction, have been fixed up in the plane of the meridian, namely, a ten feet mural circle, a ten feet transit instrument, and a zenith sector. With these instruments he is now busily occupied, following successfully in the path of Dr Bradley and Dr Maskelyne, and fully supporting the high rank of the observatory, over which he has the honor to preside.

The royal observatory at Paris was built about the same time with that of Greenwich, and is a much more splendid and costly building, nearly two millions of livres having been expended upon it; but in the construction the architect was consulted rather than the astronomer, and hardly anything about it is convenient. Nearly a century after it was erected, Cassini had to fix up an apartment, so that a heavenly body could be observed during the whole time of its being visible above the horizon. No regular series of observations have been made there and published, as at Greenwich, but many useful observations have been made at this, and at the other observatories in Paris. The catalogues of stars by La Caille, and Le Monnier, and the observations of D'Agelet and Français de la Lande may be particularly mentioned. The *Histoire Céleste Française* contains the observations of

50,000 stars by the last mentioned astronomer, the nephew of the famous Jerome de la Lande. This is an immense work, the labor of twelve years, and if these observations have not the high degree of accuracy, which is obtained in those of Bradley and Piazzi, they will still be much referred to by future astronomers, on account of the many small stars which are noticed, which are not to be found in the works of other astronomers. Of the catalogues published in other parts of the continent, the most noted are Mayer's of 992 stars observed at Göttingen; Zach's of 381 stars and the declinations of 195 stars observed at Gotha, and the new catalogue of 7646 stars observed by Piazzi at Palermo. This last is one of the greatest works of the kind in modern times. The place of each star was determined with much accuracy by taking from six to ten, and sometimes more, observations, so that the whole catalogue is founded upon nearly 150,000 observations, the reduction of which required the writing down of about 30,000,000 of figures. Bessel has lately observed 15,000 stars in a zone included between the declinations of 15° north and 15° south.

The observations heretofore mentioned required the aid of large, expensive, and stable instruments, fixed up with great accuracy in the plane of the meridian. There are, however, other observations of a different nature, which require nothing more than a good telescope and a fine pair of eyes, to notice the changes which take place upon the surfaces of the sun, planets, satellites, and comets; also to observe the double and triple stars, the milky way, the nebulæ, &c. Herschel and Schroeter were observers of this kind, they were both endowed with a sharpness of vision and a power of penetrating into space, almost unexampled in the history of astronomy. Their observations form the most important part of what is now known in this branch of the science. Herschel's acquirements in mathematics were small, and it does not seem that Schroeter's were much greater, but their exclusive devotion to this kind of observation, for which by nature they were peculiarly well qualified, enabled them to pursue it with great success. Schroeter was a magistrate of Lilienthal, and in the late war one of the French generals, Vandamme, burned and destroyed his house and observatory, and turned him almost naked into the streets. Herschel continued his labors to a good

old age, and deserves particular notice for his catalogues of the double, triple, and other multiple stars, their connexion and revolutions about each other, his great catalogues of nebulæ, discovered by his powerful telescopes, his remarks on the gradual condensation and the formation of the nucleus, &c. also for his discovery of the planet Uranus, its seven satellites, and two satellites of Saturn. He was a bold theorist, and sometimes whimsical in his notions,* but he is unquestionably entitled to the praise of having furnished many original ideas on the nature of the heavenly bodies, the construction of the nebulæ and their immense distances.

The division of labor, which contributes so much to the perfection of the arts in common life, is carried to a considerable extent in practical astronomy. Bradley, La Caille, Mayer, and Maskelyne, were observers of a very different kind from Herschel and Schroeter. The former restricted their observations to the *places*, the latter to the *forms* and *colors* of bodies. A third kind of observers may be mentioned in those, who confine themselves to the discovery and observations of comets.

Messier was an observer of this third kind. He was endowed in some respects with the same natural talents as Herschel and Schroeter, but he restricted himself almost exclusively to the discovery and observation of comets, in which he was eminently successful. He began his career with the famous comet, whose return in 1759 had been predicted by Halley, and while all the astronomers of England and France were anxiously looking out for it, he alone had the good fortune to observe it in the first branch of its orbit in January and February 1759, but by a very strange whim of his patron and *employer*, Delisle, he was not allowed to communicate the information to other astronomers, till the comet had disappeared in the sun's rays, and it was not till after its second appearance in the beginning of April, that it was observed accurately by any other person. This first attempt was followed by the discovery and observation of more than twenty comets, and his success was so great in *hunting out* these small bodies, that Lewis Fifteenth called him the *ferreter* of comets,

* An instance of this occurs in his paper submitted to the Royal Society of London, on the Solar Maculæ, in which he very gravely assumes that the *price of corn* in England might possibly be affected by the number of those spots.

(le furet des comètes,) and Messier himself was so accustomed to consider them as *his property*, that he was quite disconsolate upon being informed, that another astronomer had discovered *one*, while he was attending upon his sick wife, and unable to examine the heavens in his usual manner. Mr Pons, formerly assistant at the observatory at Marseilles, now Director of the new observatory at Marlia, has also devoted himself to the same kind of observations with great success. In the year 1822, in the course of two months, he discovered *three*, making in all twentyfive, that he had *discovered* in about twenty years. He has been rewarded with La Lande's premium for these discoveries. Dr Olbers has also been famed for his success in the discovery of comets.

Many others might be mentioned, who have made important observations in astronomy during the same period, as Bailly,* Pingré, Jerome de la Lande, Mechain, Delambre, and others, who have left imperishable monuments of their zeal and activity. A short notice of some of their works will hereafter be given, but the limits of this review will not permit a minute account of the labors of all who have formerly devoted themselves, or are now actively engaged, in making observations and calculations for the improvement of astronomy. It is sufficient to observe, that at no period was ever greater zeal evinced than at the present moment; private individuals and public associations combining their efforts to forward this useful work.

* At the commencement of the French revolution, Bailly was chosen a deputy, and afterwards appointed President of the 'Tiers Etat' of the States General, and during the struggle between that body and the court, he was the most forward to assert the popular rights, and dictated to the members the memorable oath, 'to resist tyrants and tyranny, and never to separate until they had obtained a free constitution.' Upon the capture of the Bastille he was by acclamation appointed Mayor of Paris. In the exercise of his official duties for the suppression of a mob, he ordered the soldiers to fire on the multitude, by which about forty persons were killed. This caused his popularity to decline, and when the violent party obtained the ascendency, he was condemned to death by a sanguinary tribunal, and guillotined near the spot, where he had given orders for the military to fire on the people. His sufferings were studiously protracted, and circumstances of peculiar ignominy attended his execution. A red shirt, or badge of conspiracy was put on him, and without sufficient clothing to protect him from the cold and rain, he was dragged upon a cart through the streets, with his hands tied behind him, and when, trembling from excess of cold, his executioner sneeringly said to him 'Tu trembles Bailly,' he with great firmness, instantly replied 'Oui, mais c'est du froid.' These circumstances are alluded to by Prony in his Eulogy on Pingré, in the first volume of the Memoirs of the National Institute.

The more difficult task of giving some account of the labors of those mathematicians, who have improved the science of astronomy, by their calculations of the effects of the mutual attractions of the heavenly bodies upon each other, yet remains to be performed. Many of these improvements were made little by little, at short intervals, and by different persons; others were completed at once, by efforts of genius like those which Newton manifested so frequently. The theory of gravity, as it was given by Newton in his Principia, and its application to the disturbing forces of the planets and satellites upon each other, remained without any essential improvement, till the subject was taken up by Euler, D'Alembert, Clairaut, Maclaurin, and others. Newton had proved in that work, by a geometrical method, that if only two bodies were moving in space, and mutually attracting each other by the common law of gravity, decreasing inversely as the distances, they would describe about each other, and about their common centre of gravity, some one of the conic sections, and that their velocities might be so adjusted as to cause them to revolve in a circle, or in an ellipsis of any proposed eccentricity, so that if there were no other bodies in the system except the sun and earth, the earth's orbit, in the present arrangement of the velocities and distances, would be a *perfect* ellipsis, but the introduction of another body would disturb the motion by its attraction, and the earth's orbit would no longer be elliptical, but a very irregular curve depending on the relative situations and masses of the *three* bodies. The determination of this curve is the famed *problem of the three bodies*, which has exercised the talents of the first mathematicians from the days of Newton to the present time, *without obtaining a complete numerical solution.* The difficulties of the problem were so great, that recourse was had to methods of approximation.

The first solutions of this kind were obtained about the year 1746, by Euler, D'Alembert, and Clairaut, who, without any concert with each other, had at the same time attended to the subject. The principle adopted in all their solutions was, that in the first approximation, the disturbing forces of the planets upon each other might be wholly neglected, on account of their masses being very small compared with that of the sun. In this case the planets would

revolve about the sun in ellipses, following very nearly the laws discovered by Kepler, and their positions and distances from each other at any time might be calculated in this elliptical hypothesis, with sufficient accuracy to be used in computing their attractions upon each other, and its effects by the common principles of mechanics. In calculating the orbit of any one planet, the effects of the disturbing forces of the *other* planets were computed separately, taking them *one at a time*, so that no more than *three* bodies were taken into consideration at once, namely, the sun, the disturbing planet, and the planet whose orbit was to be computed; and this complicated problem was thus reduced to the more simple one of the *problem of the three bodies*. This method of calculating separately the effects of small disturbing forces, facilitates such calculations very much, and there are but very few cases in physical astronomy, where it may not be safely used. A familiar instance of its use occurs in estimating the height of the tide, which is done by computing separately the effects of the moon and sun, and adding them together as in the conjunction, when both luminaries conspire to elevate the tide, or taking their difference in the quadratures, when the sun's force tends to decrease the tide caused by the moon.

The application of these principles to the investigation of the lunar orbit, was attended with peculiar difficulties from the greatness of the sun's disturbing force. If the sun attracted the earth and moon equally and in parallel directions, however great its *absolute* force might be, it would not produce any *disturbing* force to alter the relative orbit of the moon about the earth, which would therefore be an ellipsis. But at the time of the new moon the sun attracts the moon *more*, and, at the time of the full moon, *less* than it does the earth. In the quadratures and other parts of the orbit, the sun's attraction on the two bodies is different and in different directions, hence arises a disturbing force of the sun operating at all times on the moon, which alters the elliptical motion, and produces many important changes in the orbit; the most noted of which are the revolution of the nodes in about nineteen years, and of the apsides in nine and a half years. These motions were known in the early ages of astronomy. Three other equations of the moon's motion were discovered by ob-

servation, namely, the *evection* by Ptolemy, the *variation* and *annual equation* derived from Tycho's observations; but there are a multitude of smaller equations, which would have remained unknown without the assistance of the theory of gravity. Newton explained, extremely well, by his *geometrical method*, the motion of the *nodes* and the *variation*, but touched very slightly upon the other equations, pointing out the general results of the theory, without attempting a very accurate explanation, which, in fact, was not possible to be done by the method he used, and the differential calculus had not then been sufficiently matured to furnish a better solution.

In 1719, Halley had printed his lunar tables, founded on Newton's calculations, neglecting a few of the small equations. These tables sometimes varied 7 or 8 minutes from observations. No important improvement was made on these, till the problem of the three bodies was solved approximately by Euler, D'Alembert, and Clairaut, after the lapse of about sixty years from the time of the publication of the *Principia*. Upon applying their general solutions of this problem to this particular case, they were all surprised to find the motion of the moon's apsides, by their calculations, only half what it was known to be by observation. They had come to this unexpected result by different methods without any communication with each other, and it embarrassed them exceedingly, for, till this difficulty was obviated, it was in vain to attempt any further progress in the lunar theory, upon the Newtonian principles of gravitation, which in this case seemed wholly to fail, and this beautiful system appeared to be destined to perish, like the many which had preceded it. While the subject remained in this state, Clairaut proposed to continue the calculation of the moon's orbit, with a modification of Newton's law of the decrease of gravity, and instead of making it vary inversely as the square of the distance, to suppose it to consist of two parts, the one large and varying inversely as the square, the other small, and varying inversely as the cube of the distance. This proposal was in fact to give up the Newtonian theory. Objections were made to Clairaut's proposition by Buffon, upon metaphysical grounds, and the discussion on the main subject was continued nearly two years; but, in the beginning of the year 1749, Clairaut presented a memoir to the Academy of Arts

and Sciences of Paris, in which the origin of this difference was explained. He showed that some terms of the series for computing the motion of the apsides, which were neglected on account of their being of the order of the square of the eccentricity of the moon's orbit, and therefore supposed to be small, were, by the process of integration, divided by a very small quantity, which greatly increased their value, and nearly doubled the motion, making the theory agree with observation within a very small quantity.

This important point having been settled, it became possible to continue the investigation analytically, but the great length of the calculations caused the method to be in some respects abandoned, and while they found from the theory the forms of the equations,* upon which the period of them depends, the maxima of the coefficients were ascertained by observation, and Burg asserts, that an equation, as small as two seconds, can be detected in this way. This course, which had been partially followed by others, was more fully adopted by Mayer, in the computation of his lunar tables, with the happiest result. These tables were printed in 1770, under the direction of Doctor Maskelyne, and in 1777 he published an improved edition computed by Mr Charles Mason from Bradley's observations, which gave the moon's longitude within 30″. These were used for many years in calculating the Nautical Almanacs, occasionally making small alterations, indicated sometimes by observation, and at other times by the theory. At length, in 1806, Burg's tables were published in a new form with many improvements, for which he received a reward from the Board of Longitude of France.

* Thus the true longitude v of a planet depends on a series of terms of the form $v = m + a \sin A + b \sin B + c \sin C +$ &c., m being the mean motion of the planet, the *coefficients* a, b, c, &c. depend on the powers and products of the inclinations of the orbits, and the *angles* A, B, C, &c. depend on the mean motions, the places of the perihelion, nodes, &c. The coefficients of the terms depending on the attraction of a disturbing planet are multiplied by the fraction denoting the mass of that planet, the sun's mass being put equal to 1; and by comparing the values of these coefficients found by observation, with those deduced from the theory of gravity, the masses of the attracting bodies may be obtained. In this way the masses of Mars, Venus, and Jupiter's satellites have been ascertained tolerably well; but the results of the calculations of Laplace, Bessel, Lindeneau, and others, from the best data, present some strange anomalies, which cannot be accounted for, so that Lindeneau was induced to say, that the mass of no planet whatever is known within one fiftieth part.

In 1812, Burckhardt made other corrections, and altered the construction of the tables, so that the arguments might depend on the mean motions of the sun and moon, which rendered them far more convenient for the computation of an ephemeris. These tables were published by the Board of Longitude of France, with the title given at the head of this article. They are rather more accurate than Burg's, and give the moon's longitude generally within five or six seconds, being almost within the limits of the errors of the observations. Several of the equations in both these new tables deserve notice, from their intimate connexion with other important points of the system of the world, particularly the three following. 1. The acceleration of the moon's mean motion. 2. The equation depending on the oblateness of the earth. 3. That depending on the sun's parallax, or in other words, on the distance of the sun from the earth.

About the year 1693, Halley remarked that the comparison of ancient and modern observations indicated that the moon's motion was accelerated; or that the time of the periodic revolution about the earth was less than it was in the early ages of astronomy. A more careful discussion of the subject by Dunthorne, Mayer, and La Lande, confirmed the fact. For a long time the cause was unknown. A careful investigation showed, that the *direct* attraction of the sun and planets upon the moon could not produce such an effect. It was then suggested, that it might be produced by the resistance of the Ether; in which case the moon would continue to approach nearer and nearer to the earth, till the two bodies would come in contact, and the system be destroyed. Others supposed this acceleration might arise from the action of gravity not being instantaneous, but like light requiring time to transmit its energy. These causes would account for the acceleration, but as no other celestial phenomenon *then* indicated the existence of either of them in any sensible degree, the explanation was not deemed satisfactory, and the subject was discussed above a century before the true cause was discovered. Finally, in the year 1787, Laplace found it to be one of the simple results of the Newtonian theory, that the present apparent acceleration would finally cease, and a retardation ensue, the whole motion being nothing more than a small oscillation about the mean value, which would require some

hundreds of centuries to complete; after which the same cycle of motions would be repeated. To form an idea of the cause, it may be observed, that in like manner as the disturbing force of the sun upon the moon produces a motion of the apsides, and a change of the eccentricity of the lunar orbit, so the disturbing forces of the planets upon the earth produce a motion of the apsides, and a corresponding change of the eccentricity of the earth's orbit; but the masses of the planets being small in comparison with the mass of the sun, their disturbing forces and the effects on the earth's orbit are extremely small, in comparison with the similar effects on the lunar orbit; so that while the annual motion of the moon's apsides is above forty degrees, that of the earth's orbit is only twelve seconds, requiring at that rate above a hundred thousand years to complete its revolution; this must, however, be considered as a rough estimate, designed only to give some idea of the slowness of these motions. These equations of a long period affect only the elliptical elements of the orbit, as the place of the nodes and perihelion, the inclination of the orbit and the eccentricity, and are independent of the mutual *configuration* of the heavenly bodies; they are now, as is well known, technically called *secular equations*, to distinguish them from those which depend on the configuration, which are called *periodical equations*, because their revolutions are completed in a comparatively small period of time. A thorough investigation of these secular equations shows, that the attraction of all the planets upon the earth produces, at present, a decrease of the eccentricity of the earth's orbit of about one four hundredth part in a century, and it has been decreasing at nearly the same rate, since the most remote periods of antiquity. Moreover, the disturbing force of the sun upon the moon operates upon the moon's mean motion, and makes it different from what it would be, if that force did not act, and as the sun's force upon the moon depends on their mutual distance, which must vary with the eccentricity of the earth's orbit, it was natural to suppose, that the change of this eccentricity would affect the moon's mean motion. A strict calculation proved this to be true, and that the expression of the moon's angular motion in a series, contains a term depending on the sun's disturbing force multiplied by the square of the eccentricity of the earth's orbit, which, by taking into account the secular

decrease of that eccentricity, produces a term corresponding to the observed acceleration of the moon's motion. It was also found by Laplace, from the same theory, that the perigee and node of the moon's orbit were also subject to similar secular equations, and the truth of his calculation has been confirmed, by a discussion of some eclipses observed by the Arabs at Cairo towards the close of the tenth century.

This is one of the most noted instances of the advantage of combining theory and observation. After the acceleration of the moon's mean motion had been discovered by observation, it was at first difficult to decide satisfactorily upon the different hypotheses proposed to account for it. But when it was found by the theory, that the change of the eccentricity of the earth's orbit would not only produce this acceleration, but also similar motions of the node and perigee, which upon examination were found to correspond with the observations of the Arabs, and that these last motions would not be produced by the resistance of the ether, or the successive transmission of gravity, no doubt was left that the true cause was that change of the eccentricity.

The next point of the lunar theory, proposed to be noticed, is the small equation of the moon's longitude, depending on the place of the moon's node, first given in Mason's edition of Mayer's Tables. It was discovered by observation and found at its maximum to be $7''.7$, completing its revolution in the same time as the moon's node, that is, in 19 years. Dr Maskelyne however neglected it several years in computing the nautical almanacs, observing that it did 'not yet sufficiently appear that such an equation should arise from the theory of gravity.' But in the year 1800, Laplace discovered that this equation was a necessary consequence of the theory of gravity, and that it was produced by the spheroidal form of the earth. He also found from the same theory, that there was a similar equation in the moon's latitude. This is another instance of the mutual assistance of observation and theory. Both these equations have since been confirmed by observations. When the cause was made known it was a matter of wonder, that it had not been noticed before. Newton had proved in his Principia, that if the earth was a homogeneous *sphere*, or formed of concentric *spherical strata* of different densities, its attraction upon the moon would be exactly the same as if all

its mass were collected in one point at the centre of gravity, and thus the attraction of the millions of particles, of which the earth is composed, were reduced to the very simple calculation of the action of the same quantity of matter concentrated in that point.

This principle was adopted in all the calculations of the lunar theory, notwithstanding the earth's form was known to be *spheroidal*, and that the proposition of Newton was not strictly applicable to a body of that form, because the protuberant matter at the equator modified the attraction in some small degree, but the effect was supposed to be insensible, till Laplace showed that the two equations in question depended on the oblateness of the earth, and were directly proportioned to it. He then proposed to determine that oblateness, by comparing his theory with the result of numerous observations of the moon, suggesting that it might even be preferred to direct measures on the earth's surface, because the number of observations might be increased at pleasure, and on account of the great distance of the moon from the earth, the effect of the little irregularities of the earth's form would be hardly perceptible in the attraction upon the moon, and the result of the general figure of the earth would prevail. A careful discussion of many observations by Burg gave nearly the same oblateness $\frac{1}{305}$ by both equations, which agrees with what has been deduced from the most accurate measurements taken upon the earth's surface. Thus modern astronomers have been enabled to determine, from the moon's motion, the *spheroidal* figure of the earth, as ancient astronomers did its *round* form by the projection of the earth's shadow in eclipses of the moon. There is, however, a wonderful difference in the powers exerted in the two cases. With the ancients it was a mere exercise of good judgment, upon one of the most common of the phenomena of the shadows of bodies; with the moderns it was calling into operation the powerful resources of analysis, to estimate the forces exerted by the millions of particles of two distant irregular bodies, to notice the effects of the obliquity of their actions, the variable forces of their attractions arising from their different magnitudes, densities, and distances, to reduce them all to a single force, and then to compute its effects.

The last of the lunar equations of the moon's longitude, which was to be examined, is that depending on the sine of the elongation of the sun from the moon, from which may be computed the distance of the sun from the earth. The coefficient of this equation, being proportional to the sun's parallax, was used by Mayer to determine its value, which he found to be 7″.8, without pretending to a very great degree of accuracy. Laplace has since investigated the analytical expression, carrying on the approximation to terms of the fifth order of the eccentricities, &c. The comparison of his formula with the coefficient of the same equation, determined with the greatest care by Burg, from a very great number of observations of the moon, makes the sun's parallax 8″.56, being nearly the same as was found by Encke 8″.5776, by his elaborate investigation of all the observations of the transits of Venus of 1761 and 1769, published in his treatises, *Die Entfernung der Sonne von den Erde aus dem Venus-durchgange*,* &c. This method of finding the sun's distance from the earth by the lunar theory, is susceptible of considerable accuracy, on account of the great number of observations which can be used, so that it may even be preferred to any other known method. It must however be considered, as a very singular circumstance, that the sun's distance from the earth can be best determined by observing the *moon's longitude*, and that the same observation will also fix, with the greatest accuracy, the number of miles the earth is compressed at the poles. It shows in a striking manner the powers of modern analysis, when combined with the Newtonian theory of gravity. Attempts are still making to deduce the whole of the lunar equations from that theory alone. Laplace has done much for the attainment of this object, also Carlini and Plana have developed the analytical expression of these equations to a much greater degree of accuracy, than had ever before been done,† and it is to be hoped that it will not be long, before this important point will be attained. For after all, there is much uncertainty in referring wholly to observations, and the

* Matthew Stewart also attempted to calculate the sun's parallax from the motion of the moon's apsides, but his method is defective in principle and inaccurate in detail, and wholly unworthy of his eminent talents.

† The Baron Damoiseau lately presented to the Academy of Sciences of France, new lunar tables entitled, "Tables de la Lune formées par la soule Théorie de l'Attraction," which are recommended.

fact, that Laplace has found it necessary to change the form of the small equations of the moon's mean motion discovered by Burg, altering its period from 184 to 179 years, shows that this is not a satisfactory method of ascertaining equations of a long period of time.

When the solution of the problem of the three bodies was applied to the orbits of the sun and planets, a discussion arose upon the secular equations of the *mean motions and mean distances from the sun*, particularly those of Jupiter and Saturn, both of which appeared, by observations, to be affected by inequalities of that kind. At first it was supposed by the calculations of Euler and Lagrange, that such equations did really exist ; but a more minute investigation by Laplace, proved that all the terms of the secular equation of the mean motion and mean distance, as far as the third order of the eccentricities and inclinations inclusively, multiplied by the first power of the disturbing masses, vanish. Lagrange, by a very elegant method, soon extended this demonstration to all the powers and products of the eccentricities and inclinations, but still it was restricted to the first power of the masses. Several years afterwards, this important subject was taken up by Poisson, and by a long, but perfectly accurate demonstration, he carried on the approximations so as to include the second power of the masses. Immediately Laplace and Lagrange simplified this demonstration, and the method of Lagrange, depending on the variation of the constant quantities, was in the course of a few months improved several times by him, and became at last remarkably simple and elegant. But the struggle between these three eminent men did not here cease. For Poisson, taking up the subject again, gave another demonstration nowise inferior to any that had preceded, and he likewise proved this singular proposition, 'that the perturbations of the rotatory motion of a *solid* body of any form, arising from forces of attraction, depend upon the same equations, as the perturbations of the motion of a *single particle* of matter attracted towards a fixed centre, so that the precession of the equinoxes, and the nutation of the earth's axis, would be expressed by the same formulas, as the variations of the elliptical elements of the orbits of the planets.' Poisson did not carry on the approximation of the mean secular motions at this time, to any greater degree of

accuracy than in his former solution; but in 1816 he extended it so as to include the terms of the third order of the masses arising from those of the second order in the disturbed planet; and the demonstration is now complete for all the terms of the second order of the masses, and for some of those of the third, and so far as this approximation goes, it appears to be demonstrated, that if the *mutual attractions of the bodies composing the solar system upon each other only are noticed, that this system will be stable;* or, in other words, the mean distances of the planets from the sun, and their mean motions, will be permanent. But there may be other causes, which would destroy this stability, as for example, if the etherial space in which the bodies move be not a complete vacuum, which is probable. For, if there is no other cause operating, the light, which is passing in every direction, might cause some resistance, and that a resistance of this kind exists, can hardly be doubted, from the observations on the last return of Encke's comet, which have confirmed the former calculations of Encke, that the term of its revolution about the sun is decreasing at every appearance; the smallness of its mass rendering it susceptible of being resisted considerably, even by a fluid of great tenuity. The motions of this little body, *in its spiral course towards the sun*, will hereafter be attended to by astronomers with the greatest assiduity, for the purpose of ascertaining the resisting power and variations of density of the etherial space in the different parts of the solar system included within the limits of this orbit, whose perihelion falls within the orbit of Mercury, while the aphelion extends nearly to the orbit of Jupiter, and this small speck of matter will, on this account, become an interesting object of investigation.

Another cause of the want of stability in the solar system, may be the diminution of the sun's attractive force, from the decrease of its matter by the gradual emission of light from its surface. There are many proofs, that great changes take place in the bodies of matter composing the material universe; Herschel noticed this in some of the nebulæ. A remarkable instance occurred in the famous star observed by Tycho, which, in a moment, burst forth with a splendor exceeding that of Jupiter, and then gradually faded away till it became invisible to the naked eye, being now visible only with the assistance of a telescope. Stars, which were once visible, are

not now to be found in the places where they were formerly observed. All parts of the earth's surface likewise indicate, that at some remote period it underwent some great revolution. In fact, wherever the attention is directed, changes of place and form are perceived, with new combinations of matter, and it is natural to suppose the whole solar system will obey this general law.

It has been observed in the course of this review, that the secular equations of the apsides, nodes, eccentricities, and inclinations of the planetary orbits, are produced by the disturbing force of the planets upon each other, in like manner as the more rapid changes, in the similar elements of the lunar orbit, are produced by the disturbing force of the sun. It might also have been remarked, that the secular equation of the eccentricity of the earth's orbit is not so much noted on its own account, as for having produced the secular acceleration of the moon's motion. There is also another of these secular equations, which produces a remarkable effect, namely, the secular equation of the inclination of the earth's orbit, which, by changing the plane of the ecliptic, produces a *decrease of the obliquity of the ecliptic of about* 50 *seconds in a century.* This obliquity has been decreasing since the time of the most ancient observations, and it will continue to decrease for many centuries, then become stationary, afterwards increase till it shall attain its maximum, and then again decrease, vibrating between its extreme limits, which have been estimated not to be less than 20°, nor greater than 28°.

With respect to these secular equations of the eccentricities and inclinations of the planetary orbits, Laplace investigated a theorem from which he inferred, that these orbits would always be nearly circular, and but little inclined to the ecliptic. But there are several defects in his demonstration. He assumed the sun's mass, and the mean distance of the sun from the earth, respectively for the *unit* or measure of the masses and distances of the planets from the sun, and that the eccentricity of the orbits of any planet was estimated in parts of the mean distance of that planet from the sun. Then forming, for each planet, a product of its mass by the square root of its mean distance from the sun, and the square of the eccentricity of its orbit, he showed that the sum

of these products for all the planets would be a constant quantity, notwithstanding the mutual attractions of the bodies upon each other, and the consequent secular equations of the eccentricities. This sum being small, it follows that each separate product, for any particular planet, is and always must be small, whence he infers, that the eccentricities of the orbits of *all* the planets will be small, or the orbits vary but little from a circular form. But this by no means follows for the smaller planets, because the term of this product corresponding to Mercury, or any one of the lately discovered planets, may be small, and yet, on account of the excessive smallness of the mass of the planet, the eccentricity may be supposed excessively great, without being incompatible with Laplace's theorem. There is also another defect in his demonstration, arising from his having taken into the calculation the terms of the second order of the eccentricities, while some of the terms of the fourth order, which he has neglected, exceed those which he has retained. On the whole, it may be stated that the demonstration of Laplace is satisfactory, as it respects the three great planets, Jupiter, Saturn, and Uranus, but is not so for the other planets, whose orbits, for aught that has been proved to the contrary, may be very eccentrical. Similar objections may be made to his demonstrations, relative to the inclinations of the orbits being always small.

After it had been ascertained, that there were no secular equations in the mean motions of the planets, the discussion was continued relative to the strange anomalies, observed in the motions of Jupiter and Saturn. The tables, which would give the places of these planets with considerable accuracy for several successive years, would be quite erroneous in an interval of seventy or eighty years, and as it seemed impossible to make any tables, which would correspond with all the ancient and modern observations, it was proposed by Lambert to introduce empirical equations, which would make the tables answer for the computation of an Ephemeris for a few years. But, in the year 1786, Laplace discovered a periodical equation in the motion of both these planets, much greater than any which had before been noticed, being 48′ for Saturn, and 20′ for Jupiter, its argument being five times the mean longitude of Saturn, *minus* three times that

of Jupiter, and its period is completed in rather more than nine hundred years. The magnitude of this equation arises from the mean motions being nearly commensurable, so that five times the mean motion of Saturn is nearly equal to three times that of Jupiter. This makes the argument of these inequalities increase very slowly, *gives time for the effect to accumulate*, and though the terms on which the inequalities depend are of the third and higher powers of the eccentricities and inclinations, yet being, by the integration for the whole period of time, divisible by a term of the same order as this argument, they become very large. Several smaller equations, depending on the same cause, were also discovered by Laplace. Upon this theory Delambre computed new tables of the motions of Jupiter and Saturn, which were published in 1792, in the third edition of La Lande's Astronomy, and afterwards in Vince's collection. These tables were incomparably more accurate than any that had preceded them, but being founded in part on Flamsteed's imperfect observations, they were sometimes liable to errors of thirty seconds.

About twentyfive years afterwards the subject was taken up by Bouvard, with the assistance of the observations taken during the last interval. He computed a set of tables, founded chiefly upon observations made by Bradley, La Caille, Maskelyne, and himself, between the years 1750 and 1807. These were published in 1808, by the Board of Longitude of France, with the title at the head of this article. Bouvard compared his tables with one hundred and ten oppositions of Saturn and Jupiter, and found the error in no case to exceed thirteen sexagesimal seconds, whereas the best tables in use sixty years before, as Halley's and Lambert's, were liable to errors one hundred times as great. The errors in the tables of Saturn were sometimes more than twentytwo minutes, being equivalent to the mean motion of that planet in eleven days.

Lindeneau computed tables of the motions of Mars, Venus, and Mercury, which were published at Gotha in 1811—1813, under the titles at the head of this article. In their construction he generally excluded the observations made before the year 1750. The tables of Mars are founded on seventy observations, corresponding to twentysix oppositions of that planet, between 1751 and 1809. The tables of

Venus on about eighty observations of Bradley, between 1750 and 1755, on ninety observations of Lindeneau, Bouvard, and others, between 1804 and 1809, and on three transits of Venus over the sun's disc. The tables of Mercury are founded on eleven transits over the sun's disc, from 1677 to 1802, and on one hundred observations of Maskelyne and Piazzi, between 1775 and 1806. These tables give the places of the planets to a great degree of accuracy. Out of one hundred observations of Mercury, the error did not exceed two seconds in more than thirty observations, and in no case exceeded eight seconds. In the tables of Mercury, Lindeneau has adopted the manner of forming the arguments from each other proposed by Carlini. (*Esposizione di un nuovo Metodo di construire le Tavole Astronomiche applicato alle Tavole del Sole di Francesco Carlini. Milano.* 1810.) This consists in taking the arguments, on which the attractions of the planets depend, for the beginning of the year, and adding to each of them the *number of days* elapsed, from that time to the day for which the place of the planet is proposed to be calculated, so that in calculating an Ephemeris all these arguments have a daily increment of *one*, which renders it very easy to compute them. The tables, from which the perturbations are taken, are easily adapted to this mode of computation, by an alteration in the column in which the arguments are marked, so as to make the rate of increase correspond to the proposed form.

The labors of astronomers, on the orbits of the planets, had given to the tables of their motions a great degree of perfection, and there seemed to be but little room for improvement in this department, when an entire new field of investigation was opened by the discovery of the four small planets, Ceres, Pallas, Juno, and Vesta, some of which move in planes much inclined to the ecliptic and in orbits of great eccentricities, being moreover situated near to the orbit of Jupiter, the calculation of the perturbations of their motions becomes extremely difficult, and it may eventually require essential improvements in the present manner of making these computations, since the labor is now in some cases extremely troublesome. It requires above *four hundred equations*, (one of which, depending on the attraction of Jupiter, amounts to nearly a degree,) to compute the place of the

planet Pallas, and it takes several days to calculate accurately one latitude and longitude of that small body, not having any auxiliary tables to decrease the labor. It was fortunate for astronomy, that just before the discovery of the first of these planets, Gauss had invented his excellent method of finding the elements of the orbit from three geocentric latitudes and longitudes, which he afterwards published in his *Theoria Motus Corporum Cœlestium*, 1809. The chief excellence of this method consists in the happy selection he made of the *two unknown quantities to be investigated*, whose approximative values were to be assumed at the beginning of the operation; one of which being nearly equal to the ratio of the intervals of times between the observations, and the other nearly proportional to the product of these times. Had it not been for this improvement of Gauss, it is possible that the planet Ceres might never have been found after the conjunction with the sun, which followed its first discovery by Piazzi.* Astronomers had sought for it in vain several weeks without success, and it was not till Gauss furnished the elements of its motion, that they were able to detect it among the millions of little stars, which appear so much like it. The discovery of Ceres by Piazzi, and Pallas by Dr Olbers, may be considered as *accidental*, but this can hardly be said to be the case with respect to Vesta and Juno. The mean distances of these bodies from the sun being nearly equal, their orbits approaching near to each other in the line of intersection of their planes, and the smallness of the masses made Dr Olbers suspect, after the two first had been discovered, that they were fragments of a larger planet, which, at some distant period had exploded; and, upon this hypothesis, he recommended to astronomers to seek for more of these small bodies in the constellations Aries and Cetus, and he afterwards *actually observed Vesta in one of these constellations*. Harding had previously discovered Juno, in making his map of the small stars near which the planets Ceres and Pallas might pass. Small as these bodies are, they are now, and will continue to be, interesting to astro-

* Piazzi acknowledges this in a letter he wrote to Gauss after the planet had been found. 'Faites, je vous en prie, mes complimens et mes remercìmens à M. Gauss, qui vous a épargné beaucoup de peine et de travail, et sans lequel peut être il *ne m'aurait pas réussi de vérifier ma découverte*.'

nomers, from the great variety of equations that must necessarily exist in their motions, from the peculiar situations and forms of their orbits, and from the analytical difficulties, which may occur in the investigations.

Before the method of finding the longitude by lunar observation had been brought into successful operation, no method seemed to offer such facilities as the eclipses of Jupiter's satellites. For that reason the theory of their motions received the early attention of astronomers. About the year 1675, Roemer made the important discovery of the successive propagation of light. He was led to it from observing, that when Jupiter was near the conjunction with the sun, the eclipses happened about sixteen minûtes later than when in opposition, which he accounted for by supposing the light to require that time to be transmitted through the earth's orbit. All the satellites being affected in precisely the same manner, no doubt was left of the existence of this inequality, and it was introduced into the tables. After assuming the most probable values of the elements of the orbits of the satellites, it was found that there were several inequalities, that could not be accounted for. In 1726, Bradley suggested that the origin of them might be in the mutual attractions of the satellites, combined with a peculiarity in the arrangement of the mean motions of the three first satellites, by which their *configuration* became the same after an interval of 437 days, which might possibly produce inequalities depending on that period of time. This idea was adopted by Wargentin, in his tables published in 1746, 1754, and 1771, in which he introduced *empirical* equations, sometimes exceeding fifteen minutes of time, depending on this period of 437 days; and though they were not founded upon any accurate theory, but a vague conjecture, yet the tables agreed much better with observations, than any others which had been published, and they were therefore used in computing the eclipses, nearly fifty years before the true cause of these inequalities was distinctly pointed out, and the calculations accurately made. The celebrated Bailly, of whom we have before spoken, published several papers, and calculated some of the effects of the mutual attractions; but Lagrange, in his excellent memoir, which gained the prize of the Academy of Arts and Sciences of Paris, in 1766, was the first who gave a com-

plete theory of the motions and mutual attractions of the satellites, considering the whole of them as operating upon each other at the same time, and not restricting the calculation to *three* bodies, as had been done by all who preceded him; a point of the utmost importance in the theory of these satellites, since upon it depends the laws regulating the motions of the three inner satellites. Many numerical calculations, and a knowledge of the masses of these satellites, was still wanting. The simplest way for finding these was opened by Laplace, who, in 1784, deduced from the work of Lagrange a more limited solution, and grounded his theory on the two following laws, regulating the motions of the three first satellites, which Laplace proved would become *perfectly accurate*, by the mutual attractions of these satellites upon each other, if the conditions were *nearly* satisfied at the origin of the motions.

1. *The mean motion of the first satellite, plus twice the mean motion of the third, is equal to three times the mean motion of the second.*

2. *The mean longitude of the first satellite, minus three times the mean longitude of the second, plus twice the mean longitude of the third, is always equal to two right angles.*

Founded upon these laws, and making use of a multitude of small equations, whose form had been pointed out, and the analytical expressions given by the theory, Delambre computed an entire new set of tables, depending upon all the observations made since the year 1662. With two years incessant labor they were completed, and published in 1792, in the third edition of La Lande's astronomy. They were the first that ever gave the eclipses with much accuracy, by a correct theory, not disfigured by arbitrary empirical equations. The improvements made since by Bouvard, in the tables of Jupiter's motion, required corresponding corrections in those tables, and Delambre undertook to revise them, using the additional observations of eclipses since his first publication, and adopting the improvements in the theory contained in the fourth volume of Laplace's *Mécanique Céleste*. The result of his labors, depending upon the observations of 140 years, are the tables mentioned at the commencement of this article. They differ from the tables of 1792, in the form of some of the arguments, the

anomalies being counted from the perihelion, as well as the arguments depending on the apsides of the third and fourth satellites, some equations deduced from the theory are introduced, which were formerly omitted, the masses of the planets have been changed a little, the demi-durations of the eclipses have been decreased to correspond with observations made since the discovery of the achromatic telescope, &c. The tables of the first satellite are founded on 3439 eclipses, the second on 1100, the third on 590, the fourth on 334. All these eclipses have been recomputed by these new tables. The result of this examination showed, that the tables of the first satellite required no alteration. In those of the second, the duration of the eclipses appeared to be too great, by four or five seconds. In those of the third and fourth satellites much greater differences exist, arising from what Delambre calls, the 'frightful uncertainty of the observations of these satellites,' errors of three minutes of time being common in tolerably good observations of the third satellite; and, in some of the eclipses of the fourth satellite, he found different observers varied 7, 8, 10, 12, and 14 minutes in the same eclipse, and in one instance the difference was 29 15′. He finally concludes, that the observations of the first satellite are the only ones, that can be of any use in geography; and the general conclusion from his examination is, that the errors of his new tables are less than those of his former edition, but the gain in accuracy is hardly worth the labor it had cost him. He states also as the result of his experience, that, in finding the longitude from eclipses of the satellites, it is more accurate to compare the observations with the tables, than with corresponding observations made under a known meridian; the tables giving a mean result, which, if not absolutely certain, is at least highly probable, being grounded on all the observations of eclipses made at different times, in different circumstances, and in all countries.

The method of calculating the orbit of a comet has been greatly simplified, since the time Newton first published his indirect solution for a parabolic orbit. The return of the comet of 1759, according to Halley's prediction, drew the attention of mathematicians to the invention of formulas, for abridging the calculation of their paths, and for computing

the disturbing forces of the planets. Of the various methods proposed, the most noted are those of Lagrange, Legendre, Laplace, and Olbers, for a parabola, and Gauss for any conic section whatever. Olbers' method, which is nearly identical with that published several years afterwards by Mr Ivory, has been deservedly recommended by Gauss, as the most direct and simple of all the known methods. What formerly required the labor of days can be done by it in a few hours. Our countryman, Rittenhouse, computed the elements of the orbit of the comet of 1770, by the methods then in use, and in his letter, printed in the first volume of the Transactions of the American Philosophical Society, he says, 'Herewith I send you the fruit of *three or four days labor*, during which I have covered many sheets, and literally drained my inkstand several times.' This labor could now be completed by Olbers' method in a much shorter time. An instance of this is mentioned by Baron de Zach, who, in one of the numbers of the *Monatliche Correspondenz*, says, that his secretary, Werner, began the *reduction* of three observations of the comet of 1813 at midnight, and by half past four o'clock in the morning, he had computed the elements of the orbit. The labor is much abridged by the use of auxiliary tables, like those published by Baker,* Delambre, and Burckhardt, to which we may add the excellent table of logarithms to seconds by Taylor, a work which may be said with truth to excel all others of the kind, for its completeness and accuracy. The improvements in the calculation of the perturbation of the orbits, from the attraction of the planets, are not less remarkable. When the comet of 1759 was expected, Clairaut, with the assistance of La Lande and Madame Le Paute, calculated the perturbations of its motion by the attraction of the planets, and, with more than six months' labor, did not find its return within a month. Lately, when Encke undertook to compute the return of the comet, which bears his name, he was able, by the improved methods of Lagrange, and Bessel, without the assistance of any other person, to do the whole labor in a few weeks, with a degree of accuracy hardly to be surpass-

* A corrected edition of Baker's tables, extending to seven places of decimals, was computed by a German princess, the Duchess of Saxe Gotha, and published in 1797, by the Baron de Zach, in his edition of Dr Olbers' Treatise on Comets, and since in Delambre's Astronomy.

ed. There are now about one hundred and thirty calculated orbits of comets, of which three are known to be elliptical, namely, Halley's, which appeared in 1759, with a revolution of 75 years, Olbers' in 1815, with a revolution of 72 years, and Encke's in 1822, with a revolution of 1204 days. This last will again be visible in the northern hemisphere in the autumn of 1825. It deserves notice, that the time of revolution of this comet differs but little from that of the four newly discovered planets, being about 46 days less than that of the planet Vesta; and, as the time of the periodical revolution of the comet seems to be decreasing, it might have been, at some remote period, equal to that of Vesta. This circumstance is rather favorable to Olbers' hypothesis of the common origin of these small planets, supposing the comet to have had the same origin; and it may be observed, that Ceres has a cometary appearance, being surrounded by a luminous matter, like a nebula, so that it does not depend wholly on the reflected light of the sun to render it visible. It has been lately supposed, that most of the planets have, in a small degree, this power of self illumination. On the other hand, if the particles of light are acted upon by the common laws of gravity, as it is reasonable to believe they are, some of the largest bodies of the system might be *invisible* to us; the attractive force of such bodies being so great as to prevent the light, emitted from their surfaces, from getting beyond the sphere of their attractions. Laplace calculates that a luminous body of the same density as the sun, and 250 times its diameter, would be invisible to us, when placed at the distance of the nearest fixed star.

Comets move in every direction, and in all possible inclinations to the ecliptic, and it has been estimated by Dr Olbers, that about two of these small bodies pass annually within the earth's orbit, towards the perihelion, and that one out of 439,000,000 might possibly strike the earth; this number expressing the ratio of the whole surface of a sphere described about the sun, at the distance of the earth, to the part of the surface that would be occupied by the earth and comet. According to this calculation, a comet might possibly come in contact with the earth once in 220,000,000 of years, and it has been supposed by some, that many of the present appearances on the earth's surface have their origin in some

such cause. For if a body, moving in a plane inclined to the equator, were to strike the earth, it might change the axis of rotation; in consequence of which the fluids, and lighter substances upon the surface, would adapt themselves to the new axis. The waters would subside near the new pole, and the bottom of the ocean, with all its shells and marine productions, might there be left uncovered, while the parts near the new equator, which were before habitable, might become covered with water.

Much attention has been paid by astronomers to the determination of the figure of the earth. Newton calculated the oblateness by his theory to be $\frac{1}{230}$, supposing the earth to be a homogeneous ellipsoid of revolution. This form of the earth was assumed by him without demonstration, but it has been said with great truth, that Newton almost always *guessed* correctly, and it proved so in this instance, as has been demonstrated by several writers on the figure of the earth. Maclaurin, in 1740, was the first who gave a complete investigation of the attraction of a homogeneous ellipsoid of revolution upon any point situated *upon* or *within* its surface. His demonstration is according to the ancient geometrical method, and it is considered as one of the most beautiful specimens of that kind given in modern times. For more than thirty years this solution remained a reproach to the analytical method, which then afforded no means of solving the same problem, that would compare with it in simplicity and elegance. At length, in 1773, Lagrange, by a transformation of the coordinates, made the analytical method resume its wonted superiority. Maclaurin had also investigated the attraction of an ellipsoid upon a point situated *without* the surface, upon the continuation of either of its principal axes. This theorem was improved and extended by Legendre, Laplace, Biot, and others, so as to include all possible cases of the attraction of an ellipsoid upon a point situated *without* its surface, but the methods of demonstration were long and difficult. After this subject had been discussed above a century, by the first mathematicians in Europe, Mr Ivory, in the Philosophical Transactions for 1809, presented the subject in a new point of view, and by an ingenious transformation, made all the analytical difficulties disappear, and reduced the attraction upon a point *without* the surface, to the case of a point *within*

the surface, which had been before so completely solved by Maclaurin, and thus gave a complete solution of this problem, which had been so long a subject of interesting discussion. Mr Ivory afterwards published another paper on the subject, in the Supplement to the Encyclopedia Britannica, under the article Attraction; it vies with Maclaurin's in elegance and simplicity. He was rewarded with Sir Godfrey Copley's medal for his papers on this subject. This was the first important step the English mathematicians had for many years taken, in the calculations of *analytical* astronomy, having for many years before almost relinquished that branch of science to their continental neighbors. A new spirit has now, however, arisen among them, and the Astronomical Society of London may do much for the promotion of this science, by uniting the exertions of such men as Ivory, Brinkley, Babbage, I. W. Herschel, Woodhouse, Pond, and a multitude of other mathematicians and astronomers, who appear to be actively engaged in such pursuits.

In all these calculations, the earth was supposed homogeneous, but if it had ever been in a fluid state, the heavier parts would subside, and it would become denser towards the centre than at the surface. That this was really the case was placed beyond doubt, by two decisive experiments. The one made by Dr Maskelyne, on the attraction of the mountain Schehallien in Scotland, the other by the experiments of Cavendish, on the attraction of two large leaden balls. Dr Maskelyne found, when his quadrant was situated on the northern side of the mountain, that the plumb line by which it was adjusted was drawn a few seconds towards the south, by the attraction of the mountain; and, when it was placed on the southern side, the plumb line was drawn towards the north.* In consequence of this, the difference of the zenith distances of the same star, observed on opposite sides of the mountain, was not equal to the difference of latitude between the two places of observation, obtained by a geometrical survey, but varied from it by a quantity equal to the sum of the deviations of the plumb line. A survey of the mountain was

* Bouguer made similar observations in Peru, and Baron de Zach at Marseilles. An account of this last operation is given in Zach's work on the *Attraction des Montanges*, Avignon, 1814. 2 vols. 8vo. in which the manner of making such observations is fully explained.

taken, and its attraction upon the plumb line accurately computed by Dr Hutton, who found that the deviation was about half what it ought to have been, if the density of the mountain had been equal to that of the earth. Hence it was inferred, that the mean density of the earth must be nearly double the density of that mountain, or about five times the density of water. Nearly the same result was obtained from the experiments of Cavendish. This is also in some degree confirmed by the perfect stability, or tendency to return to a state of repose, in the waters of the ocean when disturbed. For it has been proved by Laplace, from the theory of gravity, that if the mean density of the earth was equal to, or less than, that of the waters of the ocean, the equilibrium would be unstable, and if any cause disturbed it, the tendency of gravity would be to increase the motion, and make the sea overflow its shores, and destroy the present system of the earth.

In calculating the oblateness of the earth, Newton did not take into consideration the equilibrium of the fluid upon the surface, but contented himself with the form that would result from the supposition, that the pressure of the fluid at the centre of the earth, in two small canals, drawn from that centre to the surface of the earth at the equator, and at the pole, would exactly balance each other. Bouguer showed that these canals might be in equilibrium, and yet the fluid be unstable at the surface of the earth; and he showed that it was also necessary that the force, acting on any point at the surface, should be perpendicular to that surface. Clairaut, in his theory of the earth, published in 1743, proved that there might be cases where both these conditions were satisfied, and yet the fluid be unstable, and that, for a permanent equilibrium, it was necessary that the fluid in any canal, taken at pleasure, should be in equilibrium. He investigated in that work the analytical expression of this principle, supposing the earth to be formed of concentric ellipsoidal strata, *couches de niveau*, increasing in density from the surface to the centre. He also discovered a curious theorem, by which the increase of gravity in going from the equator to the pole, determined by the length of a pendulum vibrating in one second of time in different latitudes, was connected with the oblateness of the earth; the fraction denoting the

increment of gravity being as much *above* $\frac{1}{230}$, as the fraction denoting the oblateness is *less* than $\frac{1}{230}$; so that the sum of these two quantities would be $\frac{2}{230}$, for all probable suppositions of the densities of the strata of the earth.

D'Alembert, who wrote on this subject several times, was the first who calculated the attraction of spheroids, whose meridians were not elliptical. Legendre, by an ingenious method, making use of the properties of a singular species of functions, took into consideration the case where the meridians differed from an elliptical form, and varied for different longitudes. This was also done by Laplace, who devotes the third book of his *Mécanique Céleste* almost exclusively to the theory of the earth, using functions somewhat similar to Legendre's, and founding his calculations upon a remarkable equation of partial differentials, discovered by him, by which the attraction of a spheroid, upon a point situated upon its surface, can be obtained without any integration. This equation is generally correct, but there are cases where it might fail, like Taylor's theorem, and almost all other theorems of a like general nature, when applied to some peculiar cases, in a different manner from what was usually intended. This was the case with the exception in Laplace's equation, mentioned by Lagrange, in computing the attraction of a spherical shell upon a point, situated upon its surface. The same defect was also pointed out by Mr Ivory, who had solved the general problem, by a direct process of integration, in his usual elegant manner. He also proved, that Laplace had made some deductions from his formula, which were not absolutely warrantable; but the manner in which the subject was treated by Mr Ivory evinced, in some degree, a disposition to speak too slightly of Laplace's method.

These strictures induced Laplace again to bring forward his demonstration, in the paper mentioned at the beginning of this review, read to the Royal Academy of Arts and Sciences in 1818, in which, without condescending to mention Mr Ivory by name, he says, 'Quelques géomètres ne l'ayant pas *bien saisie* l'ont jugée inexacte;' and he then goes on to show how the difficulty in question would have been avoided, if they had restricted his equation to the cases for which it had been designed. In an additional memoir published in the

Transactions for 1818, he supposes the density of the strata of the earth to increase with the pressure of the superincumbent mass, according to the suggestion of Dr Young, and he assumes as a probable hypothesis, that the ratio of the increments of the pressure and density are proportional to the density, instead of being constant as in the gaseous fluids, supposing the solid matter of the earth to resist the increase of density more powerfully than in the ratio, which prevails in the gases. This hypothesis makes the oblateness $\frac{1}{306}$, and satisfies all the known phenomena, depending on the law of the densities of the strata, namely, the variation of the degrees of the meridian and of gravity, the precession of the equinoxes, the nutation of the earth's axis, the lunar equations, depending on the oblateness of the earth, and the ratio of the mean density of the earth to that of water, which was found, by the experiments of Cavendish, to be $5\frac{1}{2}$, and, in this hypothesis, the density at the centre of the earth would be about twelve times that of water, being greater than that of lead. In this calculation, Laplace supposes the temperature of the earth to be uniform throughout the whole mass. He, however, observes that it was possible, that the heat might be greater towards the centre than at the surface, as would necessarily be the case if the earth at any period had been much heated, and was gradually cooling, conformably to his ideas of the origin of the present arrangement of the solar system, given in the last edition of his *Exposition du Système du Monde.* He discusses this point, and proves from astronomical phenomena, that this decrease has been insensible since the time of Hipparchus. His reasoning is in substance as follows. If the temperature of the earth was suddenly to decrease one degree of Fahrenheit's thermometer, its dimensions would be decreased by a quantity which, for the sake of argument, may be supposed a two hundred thousandth part, which is nearly what takes place in glass. In consequence of this, the angular velocity of rotation would be increased about one hundred thousandth part, because, *by the principle of areas*, the sum of the areas described by each particle* of the earth about its axis of rotation, would not be

* Or, in other words, the momentum of rotation found by multiplying each particle by the square of its distance from the axis of rotation, and by its angular velocity, would be the same for the whole mass *before* and *after* the change of temperature.

altered by this change of temperature, and, as the length of the day is 86,400 seconds, this length would, by this means, be decreased nearly one second. This change of dimensions would not affect the earth's mass, or its attraction on the moon, and the *absolute* time of the moon's periodical revolution, which is nearly 27 days, would not be altered; but, being measured by days, which have *decreased* in length nearly one second, that period would appear longer by 27 times that decrement, or 23 seconds; so that if the earth's temperature had decreased one degree since the time of Hipparchus, the moon's periodical revolution about the earth would appear to have increased 23 seconds. Now it has been found, by observation, after allowing for the acceleration of the moon's motion, arising from the secular change in the eccentricity of the earth's orbit, that the periodic time of revolution has not suffered any perceptible change, since the earliest observations on record; therefore no change of temperature, of any moment, could have taken place in the earth during that period.

The theory, combined with astronomical observations, has done more for the determination of the *figure* of the earth, than the actual measures of the degrees of the meridian, which have been made in several countries, with great labor and expense, but without obtaining that degree of accuracy, which was reasonably to have been expected. The degree measured in Lapland, by Maupertuis and his associates, has been found more than two hundred toises too great, by the late measurement of Svanberg.* The degree of Austria, by Liesganig, has been proved by Baron de Zach to be so very inaccurate, as to be wholly undeserving of notice. The measures at the Cape of Good Hope, Peru, and Pennsylvania, are considered tolerably accurate, but not of the first order. The late ones in France and England have

* The discovery of this mistake would have mortified extremely the vanity of Maupertuis, who, upon his return from this northern expedition, published immediately an account of it, without waiting to know the result of the operations in Peru, wishing to appropriate to himself the whole honor of the operations, to which, in fact, he had contributed but a small portion. He also caused a portrait of himself to be engraved, in a Lapland dress, with his hand resting upon the northern part of a terrestrial globe, as if he was compressing it, and for some time he was called by his countrymen, 'l'aplatisseur de la terre;' (the flattener of the earth;) instead of giving the

surpassed all others in the accuracy of the instruments, and precautions of the observers; but even these, particularly the English measurement, have not escaped animadversion, on account of their discrepancies. The most probable combination of these measures, shows that the oblateness of the earth is between $\frac{1}{300}$ and $\frac{1}{310}$, agreeing with the results of the lunar theory. It may also be observed, that this oblateness being less than $\frac{1}{230}$, proves by Clairaut's theorem, beforementioned, that the earth increases in density from the surface towards the centre, confirming the proof deduced before from other sources.

The precession of the equinoxes is intimately connected with the theory of the earth, and the oblateness of its form. Newton was the first who discovered its cause, and that, in his hypothesis of a homogeneous earth, it was produced by the attraction of the sun and moon upon the protuberant matter or excess above a sphere, supposed to be described about the polar diameter. The calculation of the precession, by the theory of gravity, is one of the most difficult of all the celestial phenomena, and the one which has been the most fruitful in mistakes. Newton's calculations for a homogeneous ellipsoid, in the Principia, contained important errors in principles and in data. These remained without detection till the year 1749, above sixty years after its publication, when D'Alembert first gave the true principles of solution in his 'Recherches sur la Précession des Equinoxes.' The general results of his solution have been confirmed by the calculations of Euler, Lagrange, and Laplace, and are now universally admitted to be true. D'Alembert proved in this work, that the sun's attraction would produce *double* the precession, which Newton had calculated, and that this mistake

glory to Newton, who had proved forty years before, from the theory, that it must be flattened. Voltaire, who was *then* the friend of Maupertuis, wrote the four following lines, placed at the bottom of this portrait.

'Ce globe mal connu, qu'il a su mesurer
Devient un monument où sa gloire se fonde;
Son sort est de fixer *la figure du monde*,
De lui plaire et de l'éclairer.'

Voltaire, at a subsequent period, when addressing himself to the members of the Academy who composed the northern expedition, says with more justice,

'Vous avez recherché, dans ces lieux plein d'ennui,
Ceque Newton connut sans sortir de chez lui.'

was nearly balanced by another in his data, in taking the moon's disturbing force considerably greater than its true value. Several other astronomers and mathematicians have since written upon this subject with various success. Bevis, Silvabelle, Walmsey, Milner, Simpson, Landen, La Lande, and Robertson, have not proceeded upon correct principles. Several of them, like La Lande, adopted Simpson's erroneous method. D'Alembert, rather vexed to find La Lande had placed his solution upon a par with Simpson's, remarked, with some testiness; 'Le fameux problême de la Précession des équinoxes, dont J'ai donné le premier la solution en 1749, a été depuis *bien ou mal résolu* par beaucoup d'autres Géometres. M. de la Lande, dans un *vaste Recueil* qu'il a publié *sous le titre d'Astronomie*, n'ayant pas distingué celles de ces solutions, qui sont défecteuses d'avec celles qui ne le sont pas, s'est contenté de les indiquer toutes *in globo*, et de dire qu'elles *ne sont pas d'accord*.' Dr Horsley, in his edition of Newton's works, adopts the prudent course of not expressing his opinion, and though fond of giving his own notes, and in many cases where no commentary was necessary, in the part treating of the precession, he very unceremoniously turns the reader over to Euler and Simpson, not wishing to decide upon so difficult a point.*

The theory of the tides, first explained by Newton, and afterwards by Maclaurin and Bernoulli, in their prize papers of 1740, has been fully examined by Laplace, in the fourth book of his *Mécanique Céleste*, and in a paper published in the Memoirs of the Academy of Arts and Sciences of Paris, for 1818. In these works he fully analyses all the effects of the change of distances, declinations, velocities or elongations of the sun and moon, and compares his theory with the observations made at Brest, during two successive periods of six and eight years; giving analytical formulas for computing the times of the tides, their heights, and all the effects arising from the change of situation and distances of the sun and moon; the whole subject being treated very much in detail, and in a satisfactory manner.

* The following is Dr Horsley's note;
Quem tamen longè alium invenerunt viri permagni Eulerus et Simpsonus nostras; quos velim Lector consulas. Ipse nil definio.

Before closing this review, it may not be amiss to mention a few of the most noted works on astronomy, in which the state of the science, as it now exists, may be found. The *Astronomie* by La Lande, in 3 vols. 4to, third edition, 1792,* is complete up to the time of its publication. It contains a description of astronomical instruments, and the methods of reducing the observations, an account of the most noted European observatories, a good treatise of spherics, with most of the formulas, used in astronomical calculations, and a collection of tables of the motions of all the planets, particularly Delambre's of the Sun, Saturn, Jupiter and its Satellites. This was the standard work to which astronomers referred for nearly half a century; nothing so complete had ever before been published. It contains a number of things that might as well have been omitted, but it is an extremely useful and interesting work for astronomers. Without having mathematical talents of the first order, La Lande, by his great zeal and devotion to astronomy, did much for its improvement. All parts of that science, which required no more than an accurate knowledge of spherics, and the elementary calculations of the perturbations of the motions of the planets, by their mutual attractions, were quite within the compass of his abilities; but when he attempted to explain and calculate the forces, which cause the precession of the equinoxes and the change of the inclinations of the lunar orbit, he laid himself open to the sneers of those, who, like D'Alembert, were offended with his excessive egotism. This foible in La Lande's character was carried to a great excess. It is to be seen in his *Bibliographie*, at every moment. In mentioning the year 1732, he remarks, 'Cette année, qui *est celle de ma naissance*, est remarquable pour l'astronomie.' In speaking of his astronomy he says, 'il a été utile en formant presque *tous les astronomes qui existent actuellement*.' He could bear the most fulsome flattery. His bust, made of Carrarian marble, having been placed in an Italian observatory, mention was made of it in a printed letter, in which it was called *il dio dell' astronomia*, (the God of Astronomy.) He thought the compliment rather extravagant,

* There was also a fourth volume relative to the tides at Brest, which was not republished with the third edition.

but was, notwithstanding, very much delighted with it. This weakness was, however, useful to astronomy. It induced him to keep up a correspondence with men of science in all parts of the world, and made him, for many years, the centre of information on all astronomical subjects.

The 'Complete System of Astronomy,' by Professor Vince, in 3 vols. 4to, 1797, 1799, and 1808, contains much useful matter, but it must be acknowledged, that it bears many marks of a crude compilation, particularly in the tables, in some of which the anomaly is counted from the aphelion, in others from the perihelion, some have all the corrections additive, others not; being copied from the works of La Lande, Delambre, and Burg, in the forms in which they were published, without taking the trouble to make much alteration, except in adapting them to the meridian of Greenwich. This mixture of different forms and systems, in the same collection of tables, may frequently lead to error, and it is to be regretted that Professor Vince did not adopt some fixed plan, and carry it fully through. The ease with which the use of the signs *plus* and *minus* is avoided in the solar tables, published by Delambre, and in those of Jupiter and Saturn, by Bouvard, makes the defect of Professor Vince's tables very apparent. Several parts of the translation of the introduction to his copy of Burg's tables are difficult to understand, without referring to the original work published by Delambre, the translation being quite imperfect and filled with errors.

Notwithstanding these defects, the work is valuable for its extensive compilation of tables of the motions of the heavenly bodies, the catalogues of the fixed stars, and the numerous auxiliary tables for facilitating most of the calculations of the practical astronomer.

The '*Astronomie Théorique et Pratique*,' by Delambre, in 1814, 3 vols. 4to, is an excellent work, but deficient in tables. All the instruments are described with the most approved methods of rectification. One of his chapters contains a good treatise on spherical trigonometry and the differential analogies, so useful in all branches of astronomy. It abounds with numerous formulas for the calculation of the effects of refraction, parallax, aberration, nutation, &c. His demonstrations are easy to follow, being quite full, without

omitting the detail of any important part. He explains the formation of the tables of the sun, moon, planets, satellites, and catalogues of the fixed stars, and gives everything which can serve to show the present state of astronomy, excepting a good collection of tables of the motions of the heavenly bodies. To supply this deficiency a person, who owns this work, would do well to procure Zach's or Delambre's solar tables, and the tables of the Planets and Satellites, whose titles are mentioned at the beginning of this review.

The *Elementi di Astronomia*, published in 1819, at Padua, by Santini, in two quarto volumes, contains the most noted theorems in spherics, and the formulas generally used in calculations of astronomy, particularly, a detailed account of the methods of Olbers and Gauss for computing the orbits of comets or planets, with Burckhardt's tables of motion for a parabola, and Gauss's tables for an ellipsis or hyperbola. It is a much smaller work than those just mentioned, does not contain the description of astronomical instruments, has but few plates, and no tables of the motions of the heavenly bodies, but is a good work of its kind.

About the year 1798, Schubert published a system of astronomy in 3 vols. 4to, in the German language, and in 1804, 1810, a smaller one, entitled '*Populáre Astronomie*,' in 3 vols. 8vo. Each volume of this latter work treats of a different division of the science, *spherical*, *theoretical*, and *physical*. It is executed in the best manner, and is well adapted to popular use. Within a short time he has reprinted his large treatise, in the French language, making many improvements in it, to adapt it to the present state of science, so that it may be considered as a new work. The well known talents of the author are a sure pledge of its excellence. Many other useful works on astronomy, of a more limited extent, might be mentioned, as those published by Biot, Woodhouse, Brinkley, and others, but the limits of this Review will not permit a full enumeration of them.

In several of these treatises an abridged history of astronomy is given, and the same is likewise to be found in various Cyclopedias and histories of the mathematics, as Montucla's, and Bossut's. There are, likewise, separate works on this subject, as Bailly's *Histoire de l'Astronomie Ancienne et Moderne*, some parts of which are beautiful, though he endea-

vours, throughout the whole work, to support his fanciful theory of the antediluvian origin of the science. It has, however, been objected to Bailly, that he took too much pains to render his writings, on scientific subjects, elegant, and that he *sometimes sacrificed the truth to his fondness for polished sentences and antitheses.* Baron de Zach, in speaking of him, makes this remark, 'Les astronomes n'ont que *trop justement reproché* a leur malhereux confrère Bailly, d'avoir été *grand phrasier*, ainsi que D'Alembert et Condorcet. Il a souvent sacrifié la vérité à une tirade, à une antithèse.'

Delambre published in 2 vols. 4to, in 1817, his ***Histoire de l'Astronomie Ancienne***, giving extracts from each author, which enable the reader to form a correct idea of the works of the most noted astronomers of antiquity. He has continued the subject in his *Histoire de l'Astronomie du Moyen Age*, in 1 vol. 4to, in 1819, and his *Histoire de l'Astronomie Moderne*, in 2 vols. 4to, in 1821, in which the same plan is pursued. This history is continued to the end of the seventeenth century, and is an excellent work. Delambre's labors were extremely useful to astronomy. The history just mentioned, in 5 vols. 4to, his astronomy in 3 vols. 4to, and the work on the measure of the arch of the meridian,* in 3 vols. 4to, form by no means, the greater part of his labors. His tables of the Sun, Jupiter, Saturn, and the Satellites of Jupiter, required several years' incessant application to complete them. He invented and simplified numerous useful formulas, and in almost everything he wrote, there was a great degree of method and elegance. As perpetual Secretary of the Institute, he made several annual reports, and delivered a number of eulogies on the deceased members, which deserve high commendation for their completeness and impartiality.

The history of the appearances of comets is given by Pingré, in his *Cométographie*, in 2 vols. 4to, which contains, also, a collection of tables and formulas, for computing their motions.

The periodical journals exclusively devoted to astronomy are numerous; as the Nautical Almanac, *Connoissance des Tems*, Bode's *Jahrbuch, etc.* The two last works contain numerous memoirs and accounts of discoveries, useful for a

* Several astronomers assisted in this measure, as Méchain, Arago, Biot, &c. but the account of their labors was drawn up by Delambre.

history of the science. The *Monatliche Correspondenz*, published by Baron de Zach from 1800 to 1813, and his *Correspondance Astronomique*, from 1818 to the present time; to which we must add the *Zeitschrift für Astronomie*, by Lindeneau and Bohnenberger, from 1816 to 1818, contain a very full and interesting account of all the discoveries and works on astronomy, during that period, so remarkable for the importance of those discoveries and the improvements in various branches of that science.

The part of astronomy, which treats of the mutual attractions of the heavenly bodies, may be studied most advantageously, in the works of Clairaut, Euler, D'Alembert, Lagrange, and Laplace. Clairaut's *Théorie de la Figure de la Terre*, is an important work. Several of his papers on the lunar theory were useful in their day, but have been superseded by the improved works of later authors. Euler's publications are extremely voluminous. Besides his separate works on all points of the system of the world, there are numerous papers of his in the transactions of the Academies of Berlin and Petersburgh, many of which are highly finished compositions, fit to be studied as models of analytical elegance. D'Alembert published several literary works, eulogies of deceased academicians, and many important articles in the *Cyclopedia*, particularly the Introduction prefixed to the first volume, also numerous memoirs in the transactions of several academies, of which he was an associate, and at intervals, he gave separately his *Opuscules*, and other mathematical and philosophical papers, in about fifteen volumes, 4to. He introduced into the calculation of problems of dynamics, an important principle by which they were all reduced to the usual calculations of statics; he also showed how to express the motions of fluids in terms of partial differentials. Euler and D'Alembert were cotemporaries, and excelled all others of their time, in mathematical genius and invention. Their talents were different, but it was not easy to decide which, on the whole, deserved the preference. D'Alembert's inventive powers were great, but he generally did not take much pains in finishing and explaining his scientific discoveries. Euler devoted himself to the improvement of the methods of analysis, and with great patience would copy a whole volume, to make a few changes in its arrangement to render it more clear, or to introduce

some small corrections and modifications; and what D'Alembert invented, Euler would frequently simplify, adorn, and explain. The course of life of these two illustrious men was very different. D'Alembert's literary acquirements, his great wit, mixed with some spice of malice, the boldness of his attacks on the most commonly received opinions in religion and government, as in some of the articles of the *Cyclopedia*, and his connexion and intercourse with Voltaire, raised up against him numerous enemies, who, by their incessant attacks, embittered his life, so that he was sometimes willing to retire awhile from this vexatious scene, and take refuge, as he says in one of his letters, in his 'peaceful geometry.' Euler's life, on the contrary, was peaceful and glorious. In his intercourse with the haughty Frederick of Prussia, at whose court he resided, as President of the Academy, he obtained at all times those attentions and civilities due to a man of his great worth, and for several years he experienced none of those ill natured sallies of wit and sarcasm, with which that monarch frequently indulged himself, at the expense of the literary and scientific men, whom he had collected around him. Upon some breach of decorum on the part of the King, Euler demanded his passports, which Frederick very reluctantly granted. Euler then accepted the invitation of the Empress Catherine, and went to Petersburgh, where he was placed at the head of the mathematical department of the Academy of Arts and Sciences of that city, and everything was done to render the situation agreeable to him and to his family. Among other honors, he had the offer of some military title, a circumstance which strongly marks the nature of the Russian government, where every one takes rank according to his military standing. It is unnecessary to say, that Euler declined the proposed honor. He continued at Petersburgh till his death, which happened in 1783, in the seventysixth year of his age. He had lost his sight several years before, but his astonishing powers of computation, by memory, remained unimpaired, and a few minutes before his dissolution, he had been employed on some calculations of the orbit of the then newly discovered planet Uranus.

Upon the decease of Euler, Lagrange remained undisputedly the greatest mathematician then living. He had

published many memoirs in the collections of several academies, with which he was associated; among them may be particularly mentioned those, in which the discovery of the *calculus of variations* is explained, a method, which extends the powers of the differential calculus, and simplifies, in a wonderful degree, the solution of a large class of interesting questions, in pure and mixed mathematics, useful in many cases of physical astronomy; also his papers on the libration of the moon, on the mutual attractions of the satellites, on the theory of functions; but, above all others, his *Mécanique Analytique*. In this work, he made a great improvement in the method of applying the principle of D'Alembert, for reducing the problems of dynamics to statics. The method used by D'Alembert was indirect, and sometimes troublesome, but Lagrange, by connecting with it the principle of *virtual velocities*, was enabled, in an extremely simple, elegant, and general manner, to reduce all the problems of mechanics to the common formulas of analysis, and the most complicated questions on the attractions of bodies were reduced to the solution of algebraical and differential equations. This work was written at Berlin, but Lagrange wished to have it printed at Paris, where it could be executed in a better style. A copy was made and forwarded to the care of the Abbé Marie, and it would now hardly be believed, that he could not, in 1788, get a printer to undertake the publication of that single quarto volume, without a guarantee to pay the expenses, in case the sale of the work should not be sufficient. The Abbé agreed to this condition, and did even more; for, at his own expense, he procured the assistance of one of the first mathematicians of Paris, Legendre, to overlook the publication, and see that it was printed correctly. The second edition of this immortal work, was published in 1811, with many additions and improvements, showing the vigor of his mind, though in extreme old age. Unfortunately for science, he did not live to complete the whole of the second volume, and a few of the last chapters are given exactly as in the first edition. This work ought to be studied frequently, by every one who wishes to learn the most approved methods of treating the science of physical astronomy. It is much easier to be read than Laplace's *Mécanique Céleste*, as it does not go into the

detail and numerical calculations, which are necessary in the application of the formulas. Lagrange succeeded Euler in the direction of the academy at Berlin, and he resided there till the death of Frederick; soon after which, in the year 1787, he was invited by the French minister to accept an appointment at Paris, where he remained till his decease, in 1813. For several years after his return to Paris, he was affected with a melancholy depression of spirits, or apathy, which made him wholly inattentive to mathematical pursuits; he said his enthusiasm was extinct; and, for two years after his *Mécanique Analytique* had been printed, his curiosity had not been sufficiently excited to cut open the leaves and look at his printed copy. A mind like Lagrange's could not, however, be unoccupied. The discoveries that had been made in chemistry, and the new nomenclature, attracted his attention; he studied that science, which had formerly appeared obscure, and was surprised to find it, to use his own expression, *as easy as algebra;* he attended also to other branches of science, to literature, and to metaphysics. The revolution, which soon after took place, again excited him, and renewed his zeal for his former pursuits; and, in the few last years of his life, he appeared with all the energy of his best days. He was a great admirer of the talents and writings of Newton, but remarked, that Newton must be considered as very fortunate, in being born at a time, when an *opportunity* was given him to explain the system of the world; a good fortune, added he, with an air of chagrin, that *one does not meet with every day.* He recommended the writings of Euler to students as models, without seeming to be aware, that nothing better could be offered for their imitation than some of his own works.

The discoveries of Laplace, who now takes the lead in mathematical acquirements, have been very numerous and important; several of them have already been mentioned. It would extend too far the limits of this review to attempt to analyse, or give a particular account of his great work, the *Mécanique Céleste*, in which all his improvements are embodied with those of the eminent men, who preceded him; the whole forming a complete and beautiful system of all that is now known in physical astronomy. Those, who take pleasure in the abstruse investigation of modern analysis, may there

find it applied, with great elegance, to the demonstration of all the principles of dynamics, to the figures and motions of the planetary bodies, satellites, and comets, and to the effects of their mutual attractions. The theorems and principles contained in this work have been explained by Laplace, in as popular a manner as the nature of the subject would admit, in his *Exposition du Systême du Monde*, which has gone through five editions, with numerous improvements. Whoever will make himself master of these works, will have no need to seek in other sources for anything relative to the principles of physical astronomy, or the application of those principles to the system of the world.

ART. V.—*Letters on the Gospels.* By MISS HANNAH ADAMS. 18mo. pp. 216. Cambridge. Hilliard & Metcalf. 1824.

THE author of these letters has long been known to the public, as a successful writer on theological subjects, and as having rendered essential service to religion, by the productions of her pen. Her *Views of Religions*, or, as she denominates it in the last edition, her *Dictionary of all Religions and Religious Denominations*, has been a popular work from the time of its first publication. It has passed through four editions, the last of which is enlarged and greatly improved. It was published in England, with a preface and additions, by Mr Andrew Fuller; and also in another form by Mr Thomas Williams, who likewise made alterations. To both these editors, Miss Adams acknowledges herself indebted, for some of the improvements of her fourth edition. This work is the best manual with which we are acquainted, for giving information respecting the religious views now entertained by Christians, and such as have prevailed in different ages, since the origin of Christianity. It has the peculiar merit of the strictest candor and impartiality; and so completely has the author divested herself of all individual prepossessions, that it may be doubted whether, from a single passage in the whole work, her own religious sentiments can be inferred. This freedom from personal bias, in exhibiting the views of others, especially on topics rarely touched without calling out private opinion, in-